Essays in corporate finance and innovation

Desai, Pranav

DOI:
10.26116/center-lis-2017

Publication date:
2021

Document Version
Publisher's PDF, also known as Version of record

Essays in Corporate Finance and Innovation

PRANAV PRADEEP DESAI

Essays in Corporate Finance and Innovation

Proefschrift ter verkrijging van de graad van doctor aan Tilburg University

op gezag van de rector magnificus, prof. dr. W.B.H.J. van de Donk, in het openbaar

te verdedigen ten overstaan van een door het college

voor promoties aangewezen commissie in de Aula van de Universiteit

op dinsdag 12 januari 2021 om 13.30 uur

door

Pranav Pradeep Desai,

geboren te Mumbai, India

Promotores:	prof. dr. O.G. Spalt (Tilburg University) prof. dr. F. Braggion (Tilburg University)
Copromotor:	dr. S. Cassella PhD (Tilburg University)
leden promotiecommissie:	prof. dr. L.T.M. Baele (Tilburg University) prof. dr. B.C.G. Dierynck (Tilburg University) dr. M. Porras Prado (Nova University Lisbon) prof. dr. L.D.R. Renneboog (Tilburg University)

Contents

Acknowledgements

Throughout the world sounds one long cry from the heart of the artist: Give me the chance to do my very best!

- Babette's Feast (1987)

This thesis is the outcome of my PhD years, which were the most challenging yet exciting years of my life. I would therefore like to thank the many different people who afforded me the opportunities and gave me the chance to do my very best in producing this dissertation.

First, I would like to thank my advisor Oliver Spalt, who repeatedly pushed me to think hard about my research, and constantly challenged me with his intellectually deep and unforgiving questions. I am also thankful to Stefano Cassella and Fabio Braggion, whose support was invaluable, especially during the final stages of completing this thesis. I am highly indebted to Elisabeth Kempf, who patiently mentored me and taught me what it means to be a productive researcher.

Next, I would like to express my gratitude to Melissa Prado, Bart Dierynck, Lieven Baele, and Luc Renneboog for providing me with detailed and insightful comments on the chapters of my thesis. I appreciate the time spent by them in reading my dissertation.

I would like to extend a warm thanks to all the Faculty members of the Finance Department for providing me with rigorous training over the past few years, in all matters research and non-research-related. I am particularly grateful to Rik Frehen who always gave me pragmatic and kind advice, and Marco DaRin who took a special interest in my development during my PhD. I must thank Loes, Helma, Marie-Cecile, Ank, and Cecile for their administrative support; Camille and Peter for their job market advice; and Emiel, Kristy, Martijn, Farah, and Yuexin for the many conversations we shared during my PhD journey. I am indebted to all the Faculty members and staff of the Department for their support during the job market process.

I am truly fortunate to have friends who gave me consistently wise advice, tolerated me at my worst, and helped me be my best self: Sanjana for always lending me a sympathetic ear and for her 'goal oriented' approach towards solving problems; Georgi for his reminders to not push myself too hard and that a life outside work also exists; Vilma for being the voice of reason and for the impromptu walks; and Ashwini for sharing her kind and intelligent views on a range of topics. I am genuinely grateful to Aditya, Shreya, and all my other friends for their patience and generosity towards me.

Last but not least, I owe my immense gratitude to my family: my mother for her tough love and for her fighting spirit which has not shown any signs of diminishing over time; my sister, for

setting an example for me, through her grit and determination in achieving all that she has; and my father for providing me with all the support over the years. Without their unconditional love and their personal sacrifices, I would never have had the opportunity to embark on this PhD journey. I, therefore, dedicate my thesis to them!

Introduction

This Ph.D. dissertation consists of three independent chapters in corporate finance and innovation. The first chapter studies how biases of patent examiners - an important set of regulators - affect their decisions to grant patents. The second chapter studies how investor attention affects analyst coverage of firms. The last chapter documents differences in how firms punish male and female inventors for “as-good-as-random” creative failures.

The first chapter titled **Biased Regulators: Evidence from Patent Examiners**, poses the question: Are regulators biased? The answer to this question has first-order economic implications. Nearly every economic activity is subject to governmental regulation and the authority to implement these rules often lies with individual regulators who possess considerable discretion in implementing them. In this chapter, I show that cognitive biases influence regulatory actions by studying the decisions of patent examiners, who decide whether or not to grant patents to inventors. By constructing a detailed dataset that links examiners to their patent approval decisions, I show that examiners are more likely to grant patents to inventors of their own race and gender. My research design exploits the random assignment of examiners to inventors, ensuring that my findings are not affected by quality of the applications. These results have implications for the patenting process in the United States and more broadly, document a new driver of the under-participation of minorities and women in innovation.

The second chapter, **Attention-Induced Information Dry-Ups**, examines how institutional investor attention affects analyst coverage of firms. While the effects of limited investor attention on corporate governance and investment have been well documented, the implications for the information provision to financial markets remain largely unexplored. This chapter provides novel causal evidence that institutional investor attention matters for analyst effort as well as the quality of their earnings forecasts. Specifically, I show that when investors “shift” their attention away from a firm, analysts correspondingly re-allocate their effort within their portfolios away from the said company. This translates into worse quality forecasts for firms with lower investor attention. These results suggest that when the demand of information reduces, the supply of information from the intermediaries is likely to “dry-up” as well.

The third chapter, **Gender Gap in Punishing Failure: Evidence from U.S. Patent Applications**, examines whether firms “punish” their female employees for creative failures to a greater extent than their male counterparts. My analysis focuses on early stage inventors who are making their first patent application to the USPTO and tracks their career outcomes after they experience an “as-good-as-random” failure. To identify these failures, I exploit the variation in the likelihood of patent examiners to grant a patent. I show that when female inventors, as compared to their male colleagues, fail to obtain a patent, they are more likely to experience a job separation, less likely to find another job, and have a higher probability of exiting innovation. Strikingly, these differences are not observed among independent inventors, that is those inventors who are not employed by a firm. An important implication of this study is that differential treatment of early stage female creative professionals by their employers after failure might explain the widely observed and documented gender differences in creative output.

Chapter 1

Biased Regulators: Evidence from Patent Examiners

1.1 Introduction

Regulatory officials matter for nearly every economic activity: they control market entry and exit, levy costly fines, and enforce penalties against firms and investors. Thereby, the distribution of resources among market participants depends critically on *how* individual regulators implement written rules. Unsurprisingly, a key tenet of fair regulatory decision-making is non-discrimination (United States Government, 2013). That is, officials are expected to decide without favoring a given social group over any other. But are regulators unbiased? Understanding whether officials exhibit in-group favoritism is important from both economic and policy perspectives. If regulators discriminate on group characteristics, their actions might be detrimental to the welfare of affected individuals. More importantly, their biased decisions may distort the allocation of resources and be costly to the economy as a whole. In spite of this, little work exists that identifies regulatory in-group biases and analyzes consequences thereof.

I aim to fill this gap by investigating the decisions of an important set of regulatory agency employees: patent examiners at the United States Patent and Trademark Office (USPTO). Studying examiners is economically relevant, because their decisions to grant patents are central to incentivizing innovation (Nordhaus, 1969). Furthermore, examiner grant decisions have first-order economic implications for startups (Gans et al., 2008; Farre-Mensa et al., 2020), firm R&D investments (Budish et al., 2015), follow-on innovation (Moser, 2005, 2013; Williams, 2013; Galasso & Schankerman, 2014; Sampat & Williams, 2019), technological progress (Merges & Nelson, 1994), and economic growth (Jaffe & Lerner, 2011).

A test of regulatory in-group biases poses two key challenges. First, matches between officials and regulated parties are often correlated to case quality. In the context of the Patent Office, examiners might choose cases which improve their career outcomes. Similarly, inventors may target officials who are more likely to decide in their favor (Fleischer, 2010). Second, regulatory decisions cannot be typically linked to individual officials. This is reflected in the lack of publicly available decision-level datasets on regulators.

I tackle these challenges using unique institutional features of the Patent Office. First, I exploit random assignment of cases to examiners within each department or "art unit". Specifically, I compare average grant rates for applications where the inventor and the examiner belong to the *same social group*, with those where they belong to *different groups*. As the determination of matches is orthogonal to application quality, this identification approach can isolate the causal effects of examiners' in-group biases on their decisions. To circumvent the second challenge, I hand-collect biographical information on examiners and inventors, and deduce their race and gender using their names. I combine this information with data that link individual examiners to their decisions during the patent application process. Overall, I observe decisions of 13,000 unique examiners on about 1.8 million applications filed by teams consisting of 2.5 million inventors between 2001 and 2018.

I present three new findings: (i) in-group biases affect examiners' patent grant decisions; (ii) examiners approve lower quality applications submitted by in-group inventors; and (iii) discrimination by examiners reduces formation of new startups as well as their likelihood of raising venture capital and going public.

I begin by showing that examiners are less likely to approve patents filed by an out-group inventor, compared to those submitted by an in-group inventor. Specifically, patent grant rate is 6 pp lower when the first inventor on the application is from another race. Relative to the average approval rate of 67.7%, this represents a striking and economically sizable 8.8% lower probability of receiving a patent. Similarly, examiners are 5 pp less likely to grant a patent when the lead inventor belongs to another gender. When examiners do issue patents to out-group inventors, they reduce the scope of novel claims therein. Together, this evidence suggests that examiner biases meaningfully impact both the applicant's chance of obtaining a new patent, as well as the content of the granted patent.

My empirical approach ensures that the results are robust to several alternative explanations. Inclusion of art unit $\times$ application year fixed effects mitigate concerns based on technological trends, racial or gender composition of art units, and inventors' choice of technology. Examiner and inventor fixed effects account for factors such as inventor skill, ability, or examiner leniency. Firm fixed effects control for time-invariant differences across firms filing patent applications. Finally, a battery of robustness checks rule out explanations based on the examiner's motivation to be hired by the firm filing on behalf of the inventor.

In the second step of my analysis, I examine the effect of biased grant decisions on quality of issued patents. Indeed, if examiners are biased, they might be more lenient towards inventors from their social groups, adopting a lower selection criteria. Hence, I pose the question: do examiners approve worse quality applications from in-group inventors? To address this question, I use measures of quality based on citations made and received by the patent. Overall, I find that patents issued to in-group inventors receive fewer citations, are less innovative, and have lower potential to encourage follow-on innovation.

The above findings suggest that examiners' biases play a crucial role in determining the distribution of patents among inventors. Given the effort involved in innovating and the cost of filing a patent, this evidence is important from the perspective of individual inventors. Yet, these findings have little power to inform us on the wider *economic effects* of biased decisions.

Hence, in the final step of the analysis, I estimate the effects of biases on firms' outcomes during different stages of their life-cycle: from startup formation to going public. Inventors matched to an examiner of a different race are 22% less likely to form new startups, relative to the average applicant. Examiner biases affect startups' access to external capital as well. Firms, whose first patent application is assigned to an examiner of a different race are 8 pp less likely to raise venture capital (VC) in the year after the patent grant decision. The effect is persistent as firms experience lower VC funding into the fifth year after the examiner decision. Finally, these startups face a lower likelihood of going public through an Initial Public Offering (IPO), thereby, restricting their access to public equity. In sum, these results indicate that replacing an in-group examiner with an out-group examiner leads to significantly worse outcomes for the applicant startups.

Collectively, this paper provides the first systematic evidence of in-group biases in decision-making by regulatory agency employees. Thereby, I make three contributions to the literature. First, I add to the literature on the effects of group-based social biases in financial settings (e.g. Wolfers (2006), Niessen & Ruenzi (2007), Kumar (2010), Kumar et al. (2015), Jannati et al. (2018)). While most of these papers focus on private economic agents such as investors, mutual fund managers, and analysts, I show that regulatory officials are susceptible to social biases as well. Public officials differ in that they neither face market competition nor are subject to discretionary firing - forces that might reduce biases in decision-making (Becker, 1957). In a broader sense, I address what Malmendier (2018) highlights as an important gap in behavioral finance: the lack of evidence on cognitive biases among *"third parties"* who shape the interactions between market participants[1]. By documenting the presence of in-group biases among patent examiners, I also provide direct empirical evidence in support of the hypotheses laid out by Hirshleifer (2008) and Hirshleifer & Teoh (2010) that regulatory decisions deviate from the rational norm.

Second, I contribute to a growing literature that studies the effect of discrimination on firms (Szymanski, 2000; Hellerstein et al., 2002; Kawaguchi, 2007; Weber & Zulehner, 2014). A closely related paper, Huber et al. (2019) shows that forced removals of Jewish firm managers due to antisemitic Nazi German policies caused reductions in stock prices and market value for large firms. My study, on the other hand, provides contemporary evidence that discrimination by individual public officials also affects firm formation and is costly for smaller, newer firms. Moreover, Huber et al. (2019) document economic losses in a setting of institutionalized anti-semitism. However, my results indicate that officials discriminate, even when they are expected to decide under a non-discriminatory norm.

Finally, my results contribute to the discussion on underrepresentation of racial minorities and women in innovation (Murray & Graham, 2007; Cook & Kongcharoen, 2010; Hunt, 2016; Bell et al., 2018). Jensen et al. (2018), for instance, focus on the Patent Office, and find that women on average face worse patenting outcomes. I point to a new factor that might drive these documented disparities: discrimination by individual examiners. My findings also have

[1] See Baker & Wurgler (2013) and Malmendier (2018) for detailed surveys of this literature

important policy implications for the debate on the efficiency of the patenting system (Merges, 2001; Lichtman, 2004; Moser, 2005; Jaffe & Lerner, 2011). Though research on this topic has focused on the design and overall welfare impact of the patenting process, I highlight the role of patent examiners in distorting the distribution of patents among inventors.

The paper is organized as follows. Sections 1.2 and 1.3 describe the patent prosecution process and my empirical approach, respectively. Section 1.4 explains the data construction in detail. Section 1.5 presents the main results on patent grant decisions. Section 1.6 sheds light on the mechanism. Section 1.7 provides evidence on effects on patent quality and startups. Section 1.8 concludes.

1.2 Institutional Background

1.2.1 Patent prosecution process

The USPTO - in its role as a federal regulatory agency - grants patents, which are economically important rights to rents from innovations. Between 2001 and 2018, approximately 8 million applications were received by the Patent Office, highlighting the scale of its operations. The patent office operates under the principle that "all patents are created equal" (Merges, 2001). Therefore, decisions on these applications are to be made by examiners in a highly-structured process and under a strictly non-discriminatory norm.

The 'patent prosecution process' begins with the submission of an application by an inventor or a team of inventors to the USPTO. These applications are sorted and forwarded to a relevant art unit, with each unit specializing in a particular technology. The Supervisory Patent Examiner (SPE) then allocates application files among examiners in the unit. Importantly, there is no systematic sorting of applications whereby certain files are assigned to particular examiners (Lemley & Sampat, 2012). This results in a quasi-random matching of applicants and examiners to each other.

Each application consists of a set of claims. For instance, in a patent application related to the iPhone, Steve Jobs and his team of inventors claimed a "computer-implemented method for use...with a touch screen display(...)" (Jobs et al., 2009). The primary task of the examiner is to decide whether to issue a patent or not, based on her evaluation of the novelty, usefulness, and non-obviousness of the inventors' claims.

To do so, the examiner searches for prior "art", that is, patents and non-patent literature such as scientific articles, which might be related to the claims made by the applicants. Often, applicants themselves include some references in their applications. The examiner combines information from her own search with references provided by applicants and determines whether the application contains new and non-obvious patentable claims. Examiners usually issue an initial "non-final rejection" citing problems with the claims. At this stage, applicants can respond by amending or removing the claims i.e. narrowing the scope of the application. The examiner can grant these modified claims and issue a patent, or issue a "final rejection". Even upon receiving a final rejection, the applicants can further modify their claims or dispute the rejection. This prosecution process continues until, eventually, the examiner issues a patent or the inventors abandon their application[2]. In addition to granting or rejecting patents, the examiner has considerable influence in determining the scope and number of claims in the issued patent.

It is additionally important to note that inventors' identity, especially, that of the lead inventor is revealed to the deciding examiner during the prosecution process.[3] While examiners do often meet inventors in-person or through telephone interviews, an important piece of information about inventors available to the examiners is their names (Jensen et al., 2018). Hence, despite the non-discriminatory policy of the USPTO, examiners can infer gender, race, and other characteristics of inventors from their names.

1.2.2 Examiner Incentives and Discretion

Patent examiners play a central role in implementing the United States Patent Law. They are highly-specialized in the subject of their respective art unit and typically, have at least a bachelor's degree in scientific disciplines, often receiving further intensive training at the Patent Office.

Examiners are hired and paid as per the "GS" i.e. the government pay scale (GS-5, GS-7, GS-9, or GS-11). They are primarily rewarded with bonuses or promotions along this scale based on the number of decision "counts" and their experience. This system does not account for the time spent on applications, the diligence in search for prior art, or even rejections. Examiners are thereby potentially incentivized towards approving applications and accumulating a higher decision count (Jaffe & Lerner, 2011; Lemley & Sampat, 2012).

Junior examiners at the USPTO are subject to supervision by more senior examiners, as their applications need to be signed by SPEs. Yet, in practice, there is limited oversight of decisions made by junior examiners. As noted by Tabakovic & Wollmann (2018), SPE's own performance reviews are based on decision counts of their respective art units and they are occupied with their own cases. Consequently, they rarely reject applications forwarded to them by examiners under their supervision. This, in turn, provides junior examiners with substantial discretion in making decisions on patent applications.

1.3 Empirical Strategy

Identifying whether patent examiners discriminate on the basis of racial or gender characteristics of inventors is empirically challenging. First, examiners' decisions on patent applications are motivated by factors beyond inventor identity, including the underlying quality of the applications, the amount of time available to the examiners, and career concerns (Lemley & Sampat, 2012; Tabakovic & Wollmann, 2018). Second, in typical regulatory settings, matches between regulated parties and decision-making authorities are not determined randomly. In the context

[2]The applicants can also file continuation or divisional applications in case of a final rejection. Additionally, since 2012, inventors have been able to appeal the examiner's decision with the Patent Trial and Appeal Board (PTAB).

[3]See example of an application form in Appendix 1.C.

of patent examination, examiners might choose applications that are easier to evaluate to increase their decision count. Conversely, inventors might tailor their applications such that they are sent to art units with a higher proportion of lenient examiners. Finally, both the nature of the prosecution process and the content of applications varies significantly, complicating comparison between cases.

My identification strategy resolves these issues by exploiting the quasi-random assignment of inventors to examiners. Specifically, I compare grant rates for applications where the inventor and examiner belong to *same social group*, with those where they belong to *different groups*. As the determination of matches between inventors and examiners is "as good as" random, I can study whether examiners treat in-group inventors more favorably by analyzing the differences between the two sets of applications. This empirical setup ensures that any disparity observed in grant rates cannot be fully explained by the quality of applications. Additionally, since the content of the application and the year of submission are chosen by applicants, I always include art unit × application year fixed effects. Thus, I compare applications submitted in the *same year to a given art unit*, controlling for time-varying differences between art units such as the proportion of lenient examiners, racial and gender composition of art units, and average grant rates.

Empirically, I estimate:

$$Grant_{ijat} = \beta mismatch_{ij} + \gamma X_{ijat} + \zeta_{at} + \epsilon_{ijat} \tag{1.1}$$

where $Grant_{ijat}$ is an indicator variable equal to one if examiner i approves the application submitted by inventor j in application year t, and zero otherwise, $Mismatch_{ij}$ is an indicator equal to one when the examiner and the first inventor belong to the same social group and zero otherwise, and ζ_{at} denote art unit × application year fixed effects. X_{ijat} is a vector of controls that includes examiner decision count, firm size, foreign priority status, and a dummy indicating whether the application is made by non-US entities.

The parameter of interest in equation (3.1) β should be negative and statistically significant, when examiners systematically show in-group favoritism. In this framework, $Grant_{ijat}$ captures the main decision made by the examiner on an application, that is whether to issue a patent or not. Thereby, it provides the benchmark for comparing applications with substantially different prosecution processes. Inventor, unless specified, refers to the first or lead inventor on the patent application. I focus on the first inventor mainly due to the salience of her name and address in the application form and in later correspondence. I show that the group characteristics of lower ranked inventors do not affect patent grants, in robustness tests in table 5.

To isolate the effect of in-group biases further, I present specifications with examiner and inventor fixed effects. Examiner fixed effects address concerns that examiner background, experience, seniority, or leniency might affect the estimates. Similarly, inclusion of inventor fixed effects rules out explanations based on time-invariant inventor characteristics such as ability or skill. I control for time-invariant characteristics of the firms to whom the application is assigned, via "assignee" or firm fixed effects. As observations are unlikely to be independent within department and for applications submitted in the same year, standard errors are reported with double-clustering at the art unit and application year level.

1.4 Data

1.4.1 Sources

Implementing the approach detailed in section 1.3 requires a dataset which (i) links individual patent examiners to their decisions on applications filed by inventors, and (ii) contains information on racial and gender characteristics of both examiners and inventors. While the Patent Examination (PatEx) dataset contains data on examiner decisions, information on the race and gender of individuals is not readily available (Graham et al., 2018a). Furthermore, for accurate classification by race or gender, I need to resolve two main issues in the PatEx dataset: first, information on examiner backgrounds, that is, their state or country of origin is not recorded and second, inventors are not uniquely identified. I overcome the first problem by hand-collecting information on examiner backgrounds from LinkedIn, Martindale-Hubbell, and web searches. I tackle the second issue by implementing a name disambiguation algorithm based on Li et al. (2014) to distinguish between inventors with similar names. Using this data on inventors and examiners, I perform a dictionary matching procedure which matches individuals to their race and gender based on their names and location. The resulting dataset contains the detailed information necessary to analyze how examiner decisions are influenced by racial and gender characteristics of the applicant. I describe the full data construction in detail below.

The starting point of assembling this dataset is the information on applications from the Patent Examination (PatEx) database. PatEx dataset contains the names and unique identifiers of examiners, decisions made by them during the prosecution process, and the names and background information of inventors filing the application. This dataset is sourced by the USPTO from application files in the Public Patent Application Information Retrieval (Public PAIR) system.

I restrict my sample to utility patent applications filed between November 2001 and February 2018, because (i) data prior to this period is incomplete and (ii) the American Inventors Protection Act ("AIPA") passed in 1999 required all non-provisional patent applications filed after December 2000 to be published. For issued patents, I consider observations between July 1995 and February 2018 to ensure complete coverage (Tabakovic & Wollmann, 2018). I retain only those cases on which decisions are made, thereby excluding 'placeholders' such as provisional, reissue, and patent cooperation treaty (PCT) applications. As the identification approach of this study relies on the quasi-random assignment of applicants to examiners, I exclude continuations, divisionals, and continuations-in-part wherein cases are often re-assigned to the same examiner who reviews the original application (USPTO, 1983).

Next, I hand-collect biographical information on examiners from LinkedIn, databases on US Government employees, and directories of patent practitioners such as Martindale-Hubbell.[4] Specifically, I collect information on the employment histories of patent examiners as well as

[4]A large number of patent examiners work for patent practitioner firms after leaving the USPTO. Patent practitioners are lawyers who file applications on behalf of the applicants.

on their prior graduate, undergraduate, and high-school education, when available.

While the PatEx dataset contains information on the inventor's address, state, and country, one key issue, as discussed above, is that it is difficult to distinguish inventors with the same name. For instance, two inventors with the name "John Smith" are not assigned unique identifiers. I disambiguate this inventor dataset using the publicly posted algorithm provided by Li et al. (2014), which uses a combination of location and names to identify inventors uniquely.

I use two dictionaries to classify race: the data on racial frequencies by last names in the 2000 and 2010 Census Surname Tables provided by the United States Census Bureau (Word et al., 2008; Comenetz, 2016) and similar information on first names from Tzioumis (2018).[5]. Using a fuzzy-matching procedure based, I am able to match 89.41% (2,516,347 out of 2,814,389) unique inventors and 81.94% of (13,215 out of 16,127) examiners with non-missing names, to their racial groups. I assign each individual to one of the six racial groups used by the United States Census Bureau if the probability of both the first and the last name belonging to that racial category is greater than 0.70. I repeat this procedure to classify gender using the state-level data on frequency of names from the Social Security Administration (SSA). As above, a name is assigned to a given gender when the percentage of names in the state belonging to that gender is above 70%. Where a name cannot be matched to the SSA dataset and for individuals based outside the United States, I use a cross-country dataset from the World Intellectual Property Organization (WIPO) (Lax Martínez et al., 2016). Overall, I am able to match 92.58% (2,605,635 out of 2,814,389) of inventors and 87.88% (14,173 out of 16,127) of examiners to their gender.

As seen in Table 1, I am able to track 1,372,257 (1,733,273) unique applications with complete information on the race (gender) of both the examiner and the first inventor, and non-missing control variables. The average patent grant rate in my sample is 67.7% . First-round grant rates are much lower at 15.2%, consistent with findings in previous studies (Jensen et al., 2018). More importantly for the purpose of this study, mismatches are not rare - with about 49.5% (33.5%) of all applications resulting in a racial (gender) mismatch.

In Panels C and D, I present the racial and gender composition of inventors and examiners. Overall, most inventors are white and male, with Asian-Americans being the second largest demographic. This especially holds true for inventors based in the United States. Similarly, examiners at the USPTO typically belong to the Non-Hispanic White racial group and are male. These findings are in line with the under-representation of ethnic minorities and women in innovation documented in existing literature (Cook & Kongcharoen, 2010; Jensen et al., 2018).

1.4.2 Sample Splits

Table 2 presents summary statistics for key variables when I split the sample by mismatches between the social group of the examiner and the first inventor. Panels A and B report the sorting results for racial and gender mismatches respectively. On average, patent grant rates are higher when the examiner and inventor belong to the same social group. More interestingly,

[5]See Appendix 1.B for detailed information on the matching process

the differences across subsamples are considerable (see Figure 1): the grant rate for a racial match is approximately 9 pp higher relative to a mismatch, while the rate for a gender match is about 3 pp higher.

Applications with a mismatch typically have lower first-round grant rates. Interestingly, inventors are less likely to appeal against an out-group examiner's decision, reducing their prospects of obtaining a patent. Moreover, examiners spend less time deciding on applications submitted by out-group inventors. These results provide preliminary evidence in favor of the hypothesis that examiners show preferential treatment to members of their own social groups. As these differences might be potentially explained by factors other than biased decisions of the examiners, I re-evaluate this evidence using more rigorous regression analysis in Section 1.5.

The main identification assumption of this paper is that cases are assigned to examiners quasi-randomly. While numerous studies have tested this assumption both empirically and using examiner surveys (Lemley & Sampat, 2012), the simple sorts in the table provide further supporting evidence. In particular, the two subsamples do not differ across key characteristics, such as, whether the examiner has a higher decision count, the applicant has a prior patent in a foreign jurisdiction, is based outside the United States, or the application is filed by a small entity.

1.5 Main results

This section presents my main results. I first establish that examiners are more likely to grant patents to inventors belonging to their racial group or gender. This result is robust to alternative explanations based on firm quality and revolving doors. Then, I focus on approved applications and document that examiners grant patents containing more claims and with wider scope to in-group inventors. Finally, I show that the biases are particularly pronounced when group identification is more salient. Together, these results suggest that in-group biases affect examiner decisions.

1.5.1 Evidence from Patent Grants

Table 3 reports the results from estimation of equation (3.1). The findings indicate that examiners are less likely to grant patents to lead inventors who do not belong to their social groups. The result in column (1) of Panel A shows that a mismatch between the race of the inventor and the examiner lowers the probability of an application being accepted by 2 pp. Even upon tightening the identification by including examiner and inventor fixed effects in column (2), a racial mismatch results in a 6 pp lower probability of a patent grant. The effect is 8.9% (=6/67.7%) of the average approval rate and is therefore economically sizable. The economic and statistical significance of the coefficient remains high when I include assignee fixed effects in column (3) to rule out explanations based on time-invariant characteristics of the firm to whom the patent is assigned. In Panel B, I repeat this analysis by focusing on gender as the social group of interest. Here, I find that the effect is similarly large ranging from 3.4 pp to 6 pp lower chance of success in case of a gender mismatch.

In Table 4, I expand upon this analysis by separately reporting grant rates for examiners belonging to different social groups. The goal of this analysis is to determine whether the above results are driven mainly by biased decisions of one group of examiners. Such a concern arises from the fact that patent office employees are predominantly white and male. In Panel A, I focus on decisions by examiners from the two largest racial groups in my sample - Non-Hispanic Whites and Asians. Reassuringly, it is evident that both white and asian examiners are more likely to favor in-group inventors. Similarly, in Panel B, male as well as female examiners are more likely to issue patents to inventors of their own gender. The consistently higher grant rates to in-group inventors by examiners across different social groups, in addition to the rich set of controls and fixed effects also mitigate concerns that application quality might be driving these results.

Overall, the findings in Table 3 and 4 suggest that in-group favoritism in examiner decisions affects one of the main outcomes of the patent prosecution process, that is the issuance of a patent.

1.5.2 Robustness

Table 5 presents a number of robustness tests. I report results for the specification in Table 3, column (3) unless stated otherwise, and suppress all control variables for brevity. As in Table 3, I present estimates for both racial as well as gender mismatches. Panel A shows results for inventors who are ranked lower in the application. Typically, a patent application is filed by a team of inventors, while much of the analysis presented above focuses on the first inventor listed in the application. Hence, I regress $Grant_{ijat}$ on mismatches between the examiner and inventors who appear second and third on the application respectively. Neither the gender nor the race of lower ranked inventors appears to have a significant effect on examiner decisions, with the size of the coefficient decreasing in accordance with the ranking of the inventor. This suggests that examiner biases are primarily driven by the group characteristics of the most salient inventor on the application.

In first line of Panel B, I employ an alternative measure of race and test the robustness of the results in Panel A of Table 3. The examiner might be able to infer the race of the inventor by the address listed directly below her name on the application form.[6] Motivated by this, I construct an alternative measure of race which is based on the racial demographics of the Metropolitan State Area (MSA), as provided by the United Census Bureau 2000 Census. Here, an inventor is assigned to the most common racial group in the MSA in which she resides. I obtain qualitatively and quantitatively similar results even upon using this location-based measure of race.

In the second line of Panel B, I consider whether team composition plays a role in obtaining a patent successfully. Thereby, I construct

Panel C addresses concerns that these results might be affected by the willingness of the examiner to seek employment with practitioners who file on behalf of the applicants (Tabakovic & Wollmann, 2018). First, I include practitioner × examiner fixed effects to rule out the

[6] See Appendix 1.C for an example of a web-based application form.

possibility that persistent links between the examiner and the law firm filing on behalf of the inventors might drive these results. This equation is estimated without examiner fixed effects. Second, I add a control variable to the baseline equation (3.1) that is equal to one if the MSA where the lead inventor is based contains a practitioner office, and zero otherwise. Both the results reported in Panel C indicate that the baseline result remains largely unchanged, despite including practitioner-related fixed effects and control variable. Therefore, it seems unlikely that an explanation based on "revolving doors" might be inducing the in-group favoritism documented in Table 3.

In Panel D, I restrict the sample to applicants with addresses in the United States. With this change in sample, the size of the coefficients increase both for racial as well as gender mismatches. A potential explanation for this change might be that examiners are more familiar with American names and thereby, more proficient at inferring their group characteristics.

In Panel E, I consider whether "ambiguous" names elicit the same biased responses from the examiner. Specifically, I estimate the specification in Table 3, column (3) on a sub-sample of first-ranked inventors, whose name belongs to a given racial or gender group with a probability between 40% and 60%. The coefficient in Panel E is much smaller in magnitude and economically insignificant. This provides suggestive evidence consistent with the hypothesis that the in-group biases of inventors are driven by name-based stereotyping.

Finally, in Panel F, I examine the role of inventor experience and applicant team characteristics. In the first line, I include an additional control for inventor experience, which is the natural logarithm of the number of patents successfully obtained by the inventor prior to the focal application. In the second and third rows, I add controls for racial and gender composition of the team as well as for the prior patents obtained by *other* inventors on the applicant team. In the last row, I include fixed effects for inventor teams. Overall, the results remain unchanged even upon including a wide range of controls to account for other inventors with whom the applicant produces the patent.

1.5.3 Patent Prosecution Outcomes

While patent grant is an important measure of the success of an application, examiners have considerable influence over other outcomes during the process as well as on the final contents of the patent itself. This section studies the impact of being assigned an out-group examiner on early-stage rejections, time spent on deciding the outcome, as well as the claims allowed in the issued patent.

Table 6 reports the results. I begin by considering non-final rejections by examiners. Though non-final rejections are common[7], the results are still informative in terms of the additional effort required by inventors in the prosecution process. The second outcome is whether inventors are likely to request a re-examination after receiving a final rejection. Re-examination is one of the main methods by which inventors can appeal the examiner's decision. Hence, this specification provides an insight into whether the inventor decides to continue with the patenting process after the final rejection. The third outcome I consider is the time spent by the examiner before

[7]In my sample, the rate of first round or non-final rejections is 84.8%.

issuing the final decision. Thereafter, I focus on three variables which measure the quantity and quality of claims in the issued patent. Reducing the number of claims in a patent, as reported in column (4), might lower its economic value (Merges & Nelson, 1990). Reduced claims also lengthen patents in terms of number of words as inventors have to cite more prior art (Quinn, 2015). Estimates on changes in word count are reported in column (5). In column (6), I use patent scope - a measure of the number of words in the claims normalized at the art unit level (Kuhn & Thompson, 2019). A higher patent scope corresponds to fewer words in claims.

All results in Table 6 consistently indicate that a racial or gender mismatch results in worse patenting outcomes for the inventor. Specifically, examiners are less likely to issue a patent in the first round to out-group inventors, while spending more time in decision-making. The latter result is in stark contrast to the sorting estimates presented in Table 2. However, it is important to note that the regression-based estimates are more rigorous, in that, they control for time-invariant examiner and inventor-level factors. More importantly, inventors rejected by an out-group examiner are less likely to file an appeal, thereby reducing their chances of pursuing a patent after receiving a final rejection. In addition, a mismatch shapes the nature of claims in the final patent. Both the number of claims as well as the scope of the patent reduce, thereby, reducing its economic value for the applicant. As a broader patent is more likely to be sold by the inventor, these changes might affect the overall probability of re-sale as well (Kuhn, 2016). Together, results in Table 3 and Table 6 indicate that examiner biases affect patent decisions at both the extensive and intensive margins.

1.6 Mechanism

The preceding section establishes that examiners treat inventors who are not from their in-group less favorably. In this section, I attempt to provide direct evidence in support of the interpretation that these results can be explained by examiner in-group favoritism. Specifically, I test a central prediction of the social identity theory, that is, increased salience of group membership intensifies in-group biases (Mullen et al., 1992). I also study whether examiners are more likely to be biased when their cognitive load is higher.

First, I construct a variable - racial conflict, which proxies for the public attention to racial discord in the United States. I begin by constructing a variable which is equal to one when the negative media mentions of China are above median as compared to the sample period and zero otherwise. A mention is classified as negative, when the term China appears with a negative term in the headline of the newspapers *Wall Street Journal*, *New York Times*, *Los Angeles Times* and *New York Post* with negative terms being obtained from the Harvard IV TagNeg dictionary. This measure is motivated by the previous findings that bias against Asian-Americans is heightened during periods of increased negative mentions of China on social and traditional news media (Darling-Hammond et al., 2020). Second, I identify whether examiners are from a state which had passed Jim Crow laws in the past. Jim Crow laws were a collection of laws passed primarily at the state or the county-level which legally enforced racial segregation. My main conjecture is that individuals raised in states with a history of racial conflict might pay greater attention to the racial characteristics of the applicants, motivated by prior findings of the literature that implicit racial biases are higher in these regions (Payne et al., 2019). I define the variable - segregation state as being equal to zero, if the examiner's earliest education occurred in a state with a history of having passed Jim Crow Laws and zero otherwise.

Finally, I test an explanation based on the examiner's cognitive load, as measured by her workload during the application year. I test the hypothesis that examiners are more likely to base their decisions on the salient characteristics of the case, such as the racial characteristics of the inventor's name, when they have less time to make their decision (Bordalo et al., 2015). Here, I define examiner workload, as an indicator variable equal to one if the number of cases pending with the patent examiner is above the median of her art unit in the application year, and zero otherwise.

Table 7 presents analysis of the mechanism, wherein I interact these three measures with racial mismatch. I observe that examiner in-group favoritism intensifies with increased attention to racial conflict. Similarly, examiners from segregation states which have a history of racial conflict, are more biased in their decision-making. On the other hand, increased case workload does not appear to have an effect on examiner decisions when there is a racial mismatch. These results further support my interpretation that the differences in patent outcomes documented in this paper are driven by in-group biases. In contrast, the amount of time or cognitive resources available to the examiner cannot explain these results.

1.7 Costs of Biased Decisions

The efficient allocation of patents is central to innovation and therefore, to overall economic growth (Jaffe & Lerner, 2011). The evidence presented in this paper so far documents the distortion in allocation of patents due to examiner biases. Still, it is unclear whether these decisions have any wider economic effects. The goal of this section is therefore to get a sense of the economic costs imposed by biased examiner decisions.

1.7.1 Patent Quality

I begin by analyzing the impact of biased grant decisions on the quality on patents, as measured by their citations. Patent citations are typically associated with higher market value (Kuhn & Thompson, 2019) and any reduction in patent quality might consequently lower their economic value as well.

Table 7 provides insights into these effects by regressing measures of patent quality on examiner-inventor mismatches. I first consider the number of citations received by a patent in five years after issuance. This measure captures the impact of a patent in terms of generating future innovations. My findings indicate that a patent resulting from a racial mismatch garners 0.93 more forward citations - an effect which is economically large at 20.23% (=0.093/0.459%) of the unconditional mean. The effect is similarly large for a gender mismatch at 10.46% (=0.048/0.459%) of the average.

Similar patterns hold in the next three columns. On average, patents issued to an out-group inventor are of better quality, in terms of the citations made to previous art, as well as generality, and originality of the patent. The estimates in columns (3) and (4), are especially important, given that generality and originality estimate the impact in terms of spurring innovation in a wider number of fields and technological novelty relative to existing innovations, respectively (Trajtenberg et al., 1997).

One potential interpretation of the above evidence is that examiners apply a lower threshold to members of their own social groups, by approving worse quality and economically less valuable patents. Another way to interpret these differences in patent quality is that inventors with only the highest quality innovations proceed with their applications, when faced with an out-group examiner. This view is consistent with self-selection in entry by minority groups due to discrimination (Kumar, 2010). However, distinguishing between these two interpretations is not possible using the average effects reported in Table 7. Nonetheless, these results do indicate that patent quality is negatively affected due to in-group favoritism by examiners, as lower quality and thus, economically less valuable patents from in-group inventors are accepted.

1.7.2 Effects on Startups

Beyond affecting the quality of patents, biased grant decisions might negatively impact the participants in the patenting process as well. So, I shift the focus of my analysis to consider the impact on the patenting firm during different stages of its life-cycle: from formation to raising venture capital and then, going public. Patents are important for startups while accessing external capital, whether through venture capital or through public offerings. The information frictions involved in raising capital are especially high for newer firms. Patents ease this process for startups, as they can either pledge their patenting rights as collateral while securing loans or signal their quality through successful patent applications (Farre-Mensa et al., 2020).

I follow Farre-Mensa et al. (2020) in defining a startup, using data from Thomson One VentureXpert. I begin by dropping firms based outside the United States and not-for-profit entities like academic institutions and government agencies. To exclude large publicly listed firms and their subsidiaries from the sample, I match firm names with company names in CRSP-Compustat dataset and remove firms with a history of patenting, retaining mainly applicants assigned the "small business entity" status by the USPTO. Using this newly constructed sample, I identify the inventors who are involved in setting up new firms by matching inventor names to names of individuals involved in founding startups. I also augment this dataset by hand-collecting information on startup founders from LinkedIn.

I estimate equation (3.1), replacing the dependent variable with measures of firm outcomes. I begin by focusing on the probability that an inventor whose first application was assigned to an out-group examiner, starts a new firm in the five years after the examiner decision. This helps me study the effect of a mismatch on formation of new firms by inventors. Next, I study whether startups' ability to raise venture capital is affected by a group mismatch on its first application. Finally, I examine whether a startup's transition to becoming a publicly listed firm through an Initial Public Offering (IPO) is affected by inventor-examiner mismatches.

Table 9 reports the results. A racial mismatch reduces the inventors' likelihood of setting up a new firm by 1.1 pp. The economic magnitude of this effect is large at 22% (=1.1/5%) of the unconditional mean. In other words, replacing an in-group examiner with an out-group examiner significantly reduces the probability of an inventor becoming a startup founder. Columns (2) to (6) highlight the impact of inventor-examiner mismatches on startups' likelihood of raising venture capital. Specifically, when the first inventor on the application filed by the startup belongs to a different race or gender than the examiner, the firm is less likely to obtain venture capital. This effect is both statistically significant and persistent, lasting up to five years after the decision on the first application. In column (7), I show that a mismatch has an impact on the firm's ability to raise external capital through an IPO by 0.6 pp. While this effect might seem small *prima facie*, only 0.6% of the firms in my sample file an IPO between 2001 and 2018. As the matches are determined quasi-randomly, other factors such as quality of the startup or the application cannot fully explain these patterns. The combined evidence presented in Tables 7 and 9 suggest that examiner biases have economic costs beyond simply reducing patent grant rates for out-group inventors: they also reduce patent quality and result in measurably worse outcomes for the startup founders, newer firms, and more mature startups aiming to raise external capital.

1.8 Conclusion

I show that in-group biases affect the decisions of an important set of regulatory agency employees: patent examiners. Using a novel hand-collected detailed dataset that links patent examiners and inventors to their race and gender, I show that examiners are less likely to grant patents to inventors who do not belong to their social group. My identification approach exploits the quasi-random assignment of examiners to inventors, ensuring that differences in quality of the applications cannot explain the results. Moreover, the findings are consistent with the predictions of social identity theory: biases intensify when the salience of group membership increases. I further show that biased decisions by examiners have effects on patent quality, startup formation, and the likelihood of new firms raising venture capital and going public.

This paper contributes to the vigorous debate on the effects of regulations by focusing on the decision-making officials who implement these rules. To the best of my knowledge, this is the first study to provide systematic causal evidence of discrimination in decisions by an important set of regulatory agency employees: patent examiners. If sophisticated regulatory decision-makers such as examiners are susceptible to biased decision making, it is likely that these biases might influence the actions of other economically important regulators as well and have important consequences both for the distribution of resources and rate of economic growth. I leave exploring these effects in other settings to future research.

FIGURE 1
Patent Acceptance Rates by Examiner-Inventor Matches

This graph plots the share of patent applications resulting in a grant for matches and mismatches by race and gender between the patent examiner and the first inventor on the application. A mismatch (match) is an application where the examiner and the first inventor both belong to different (same) race or gender. Asterisks ***, **, and * indicate statistical significance of the difference between groups at the 1%, 5%, and 10% levels respectively, with standard errors that allow for double-clustering at the art unit and application year level.

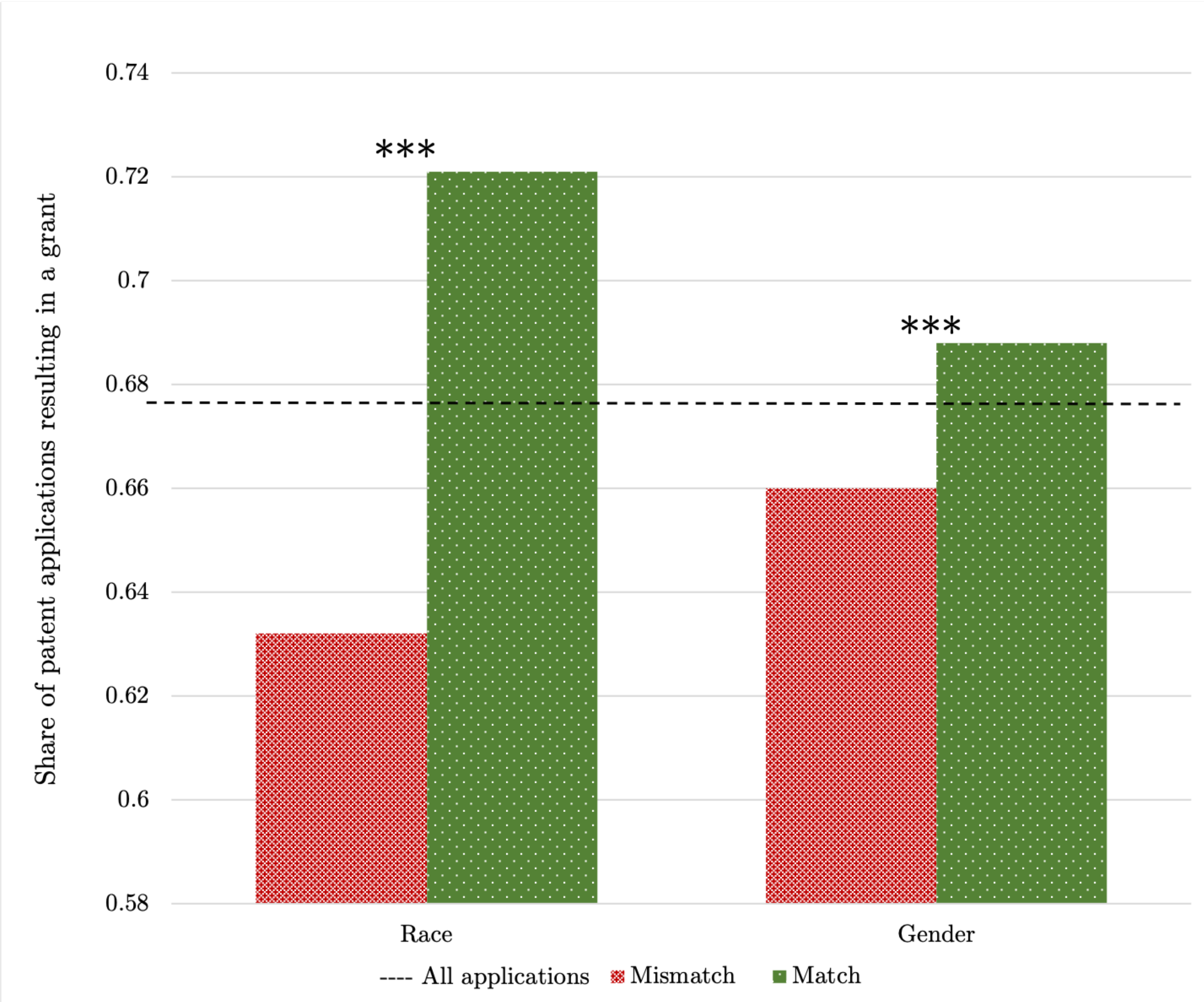

TABLE 1
Summary Statistics

This table presents summary statistics for key variables. The sample in Panel A consists of all patent applications between November 2001 and February 2018, while the sample in Panel B consists of all issued patents between July 1995 and February 2018. In Panels C and D, the demographics of individual examiners and inventors are presented. All variables are defined in Appendix A.1.

Panel A: All Applications

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent Variables						
Patent grant	1,733,273	0.678	0.467	0.000	1.000	1.000
First round grant	1,733,273	0.152	0.359	0.000	0.000	0.000
Appealed	1,733,273	0.247	0.431	0.000	0.000	0.000
Decision time	1,733,273	1078.720	444.832	743.000	1009.000	1348.000
Independent Variables						
Racial mismatch	1,372,357	0.495	0.500	0.000	0.000	1.000
Gender mismatch	1,733,273	0.335	0.472	0.000	0.000	1.000
Control Variables						
Foreign application	1,733,273	0.722	0.448	0.000	1.000	1.000
Small entity	1,733,273	0.237	0.425	0.000	0.000	0.000
Examiner experience	1,733,273	6.791	1.397	6.064	7.011	7.808
Foreign priority	1,733,273	0.550	0.498	0.000	1.000	1.000

Panel B: All Issued Patents

	N	Mean	St. Dev.	0.25	Median	0.75
Dependent Variables						
Δ number of claims	802,932	0.705	0.976	0.000	0.311	0.973
Δ words in claims	802,956	0.017	0.419	0.000	0.000	0.000
Patent scope	535,522	-0.035	1.033	-0.462	0.169	0.637
Patent Quality						
Forward citations	1,122,510	0.459	1.346	0.000	0.000	0.000
Backward citations	1,389,502	8.911	14.152	3.000	6.000	10.000
Generality	1,122,510	0.419	0.436	0.000	0.333	1.000
Originality	1,389,502	0.520	0.342	0.222	0.600	0.806

Panel C: Racial Demographics of Inventors and Examiners

	N	White (%)	Hispanic (%)	Black (%)	Asian (%)	American Indian (%)	Mixed Race (%)
Inventors (All)	2,516,347	60.99	2.70	0.07	36.24	0.00	0.00
Inventors (Only US)	1,185,781	91.60	2.15	0.07	6.17	0.00	0.00
Inventors (First-ranked)	960,117	59.58	2.50	0.07	37.84	0.00	0.00
Inventors (US and first-ranked)	420,967	91.84	2.05	0.07	6.05	0.00	0.00
Examiners	13,215	79.01	2.68	0.39	17.93	0.00	0.00

Panel D: Gender of Inventors and Examiners

	N	Male (%)	Female (%)
Inventors (All)	2,605,635	83.99	16.01
Inventors (Only US)	1,466,188	86.30	13.70
Inventors (First-ranked)	892,222	85.56	14.44
Inventors (US and first-ranked)	574,307	87.50	12.50
Examiners	13,173	70.73	29.27

TABLE 2
Sample Splits

This table reports sample splits by examiner-inventor matches for the main variables of interest. Panels A and B present splits for matches between examiner and first inventor on the application based on race and gender respectively. I report the sample average (All), the average when both examiner and inventor belong to the same racial or gender group (Match), and the average when the examiner and inventor do not belong to the same group (Mismatch), as well as the t-statistic for the difference between the two subsamples. All variables are defined in Appendix A.1. Reported t-statistics are based on standard errors that allow for double-clustering at the art unit and application year level.

Panel A: Matches by Race (All Applications)

	All	Mismatch	Match	t-stat
Dependent Variables				
Patent grant	0.677	0.632	0.721	2.53
First round grant	0.154	0.146	0.162	1.81
Appealed	0.245	0.241	0.248	4.09
Decision time	1074.289	1056.910	1091.350	10.55
Control Variables				
Foreign application	0.743	0.852	0.636	-0.00
Small entity	0.237	0.198	0.275	1.43
Examiner experience	6.799	6.781	6.816	1.25
Foreign priority	0.570	0.684	0.459	-0.02
N	1,372,562	679,904	692,658	

Panel B: Matches by Gender (All Applications)

	All	Mismatch	Match	t-stat
Patent grant	0.678	0.660	0.688	9.21
First round grant	0.152	0.149	0.154	5.86
Appealed	0.247	0.245	0.248	3.22
Decision time	1078.720	1073.620	1081.287	1.50
Control Variables				
Foreign application	0.722	0.731	0.717	-1.01
Small entity	0.237	0.251	0.230	-0.53
Examiner experience	6.791	6.750	6.811	1.63
Foreign priority	0.550	0.554	0.548	-1.12
N	1,733,273	581,090	1,152,183	

TABLE 3

Examiner In-group Biases and Patent Grant Decisions

This table regresses patent grant decisions on group mismatch between examiner and first inventor in the application. Panels A and B report results from mismatches by race and gender respectively. The dependent variable is an indicator variable equal to one if the patent application resulted in a final grant decision and zero otherwise. Racial (gender) mismatch is an indicator variable equal to one if the race (gender) of the first inventor on the application is not the same as that of the patent examiner, and zero otherwise. All variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the art unit and application year level.

Panel A: Matches by Race

	Patent Granted		
	(1)	(2)	(3)
Racial mismatch	-0.020	-0.060	-0.051
	(-2.09)	(-5.93)	(-3.38)
Foreign priority	-0.044	-0.019	-0.006
	(-9.13)	(-3.67)	(-1.51)
Foreign application	-0.017	-0.012	-0.007
	(-4.50)	(-5.16)	(-1.32)
Small entity	-0.166	-0.129	-0.122
	(-29.86)	(-17.13)	(-17.98)
Examiner experience	0.085	0.100	0.101
	(33.65)	(24.47)	(24.52)
Art unit $\times$ year FE	Yes	Yes	Yes
Examiner FE	No	Yes	Yes
Inventor FE	No	Yes	Yes
Assignee FE	No	No	Yes
N	1,372,030	939,495	612,074
R^2	0.20	0.50	0.62

Panel B: Matches by Gender

	Patent Granted		
	(1)	(2)	(3)
Gender mismatch	-0.060	-0.050	-0.034
	(-3.47)	(-2.20)	(-3.12)
Foreign priority	-0.039	-0.020	-0.018
	(-8.97)	(-4.60)	(-3.47)
Foreign application	-0.023	-0.014	-0.012
	(-6.54)	(-7.43)	(-5.30)
Small entity	-0.167	-0.131	-0.129
	(-30.14)	(-17.01)	(-16.41)
Examiner experience	0.085	0.100	0.100
	(33.37)	(25.45)	(25.00)
Art unit × year FE	Yes	Yes	Yes
Examiner FE	No	Yes	Yes
Inventor FE	No	Yes	Yes
Assignee FE	No	No	Yes
N	1,733,273	1,207,482	881,461
R^2	0.11	0.20	0.50

TABLE 4

Examiner In-group Biases in Patent Grant Decisions by Social Group

This table repeats the analysis in Table 3, but reports the results separately by group identities. Panel A estimates the regression on the subsample of lead inventors and examiners who are either White or Asian. The dependent variable is an indicator variable equal to one if the patent application resulted in a final grant decision and zero otherwise. White (Asian) examiner is an indicator variable equal to one if the examiner belongs to the White (Asian) racial group. Similarly, White (Asian) inventor is an indicator variable equal to one if the first inventor on the application belongs to the White (Asian) racial group. Panel B repeats this analysis with gender as the social group of interest. Male (Female) examiner is an indicator variable equal to one if the examiner is male (female). Male (Female) inventor is an indicator variable equal to one if the first inventor on the application is male (female). *t*-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the art unit and application year level.

Panel A: Matches by Race

	Patent Granted		
	(1)	(2)	(3)
White examiner	-0.005		
	(-1.46)		
Asian inventor	-0.003		
	(-1.88)		
White examiner × Asian inventor		-0.012	
		(-5.00)	
Asian examiner × White inventor			-0.090
			(-3.90)
Foreign priority	-0.043	-0.018	-0.018
	(-8.16)	(-3.38)	(-3.37)
Foreign application	-0.016	-0.011	-0.011
	(-4.91)	(-4.29)	(-4.28)
Small entity	-0.166	-0.129	-0.129
	(-29.57)	(-17.24)	(-17.24)
Examiner experience	0.085	0.100	0.100
	(33.62)	(24.63)	(24.63)
Art unit × year FE	Yes	Yes	Yes
Examiner FE	No	Yes	Yes
Inventor FE	No	Yes	Yes
N	739,504	680,441	680,441
R^2	0.19	0.50	0.50

Panel B: Matches by Gender

	Patent Granted		
	(1)	(2)	(3)
Male examiner	0.013		
	(3.59)		
Female inventor	-0.043		
	(-15.31)		
Male examiner × Female inventor		-0.010	
		(-2.19)	
Female examiner × Male inventor			-0.080
			(-2.48)
Foreign priority		-0.019	-0.010
		(-4.16)	(-4.23)
Foreign application		-0.013	-0.014
		(-6.79)	(-6.74)
Small entity		-0.131	-0.123
		(-17.15)	(-17.20)
Examiner experience		0.100	0.103
		(25.51)	(22.36)
Art unit × year FE	Yes	Yes	Yes
Examiner FE	No	Yes	Yes
Inventor FE	No	Yes	Yes
N	1,732,799	1,207,482	1,207,482
R^2	0.11	0.49	0.58

TABLE 5

Robustness

This table presents robustness tests. The baseline regression refers to specification (3) from Panels A and B from Table 3. For brevity, I only report the main coefficients of interest without presenting the coefficients of the control variables. Panel A tests grant rates by mismatches between the examiner and lower ranked inventors on the patent application. Accordingly, the next two lines present the coefficients for mismatch between the examiner, and second and third ranked inventors respectively. In Panel B, I use an alternative measure of race. Here, an inventor is assigned to the race which constitutes the majority in the Metropolitan Statistical Area (MSA) where she resides, as obtained from the US Census Bureau. Thereby, a mismatch is defined as one if the race of the first-ranked inventor and the examiner do not match and zero otherwise. In Panel C, first line, I add practitioner × examiner fixed effects, where practitioner refers to the patent practitioner who represents the inventor team. Here, I estimate the equation without examiner fixed effects. In the second line, I add a control which is defined as one if the patent practitioner office is in the same state as the first inventor on the application. In Panel D, I restrict the sample to first-ranked inventors who reside in the United States. In Panel E, I restrict the sample to those individuals whose name is ambiguous. An ambiguous name is defined as one which belongs to a given racial group or gender with a probability between 40% and 60%. Panel F re-estimates the baseline specification with additional controls. In the first line, the specification is estimated with a control for inventor experience, which is defined as the natural logarithm of the number of patents successfully obtained by the inventor prior to the focal application. In the second line, a control is added for team group characteristics, which is the percentage of inventors on the team who belong to the examiner's out-group, excluding the first inventor. In the third line, a control for team experience is included, which is defined as the average of natural logarithm of number of patents successfully obtained by inventors prior to the focal application, excluding the first inventor. In the final line, I add team fixed effects.

	Matches by Race			Matches by Gender		
	Coeff.	*t*-statistic	*N*	Coeff.	*t*-statistic	*N*
Baseline	-0.060	(-5.93)	939,495	-0.050	(-2.20)	1,207,482
Panel A: Alternative inventor rank						
Second ranked inventor	-0.005	(-1.33)	427,221	-0.003	(-0.82)	699,521
Third ranked inventor	-0.002	(-0.54)	277,274	0.007	(1.35)	304,689
Panel B: Alternative measures of race						
Zip-code racial concentration	-0.310	(-6.21)	939,495			
Panel C: Revolving doors						
Practitioner × examiner FE	-0.071	(-4.55)	396,571	-0.050	(-3.21)	392,999
Practitioner offices	-0.060	(-5.07)	396,572	-0.051	(-3.26)	392,999
Panel D: Estimation methods						
US applicants only	-0.072	(-4.18)	690,499	-0.121	(-4.94)	976,151
Panel E: Alternative thresholds for names						
Ambiguous names	-0.009	(-1.04)	130,094	-0.012	(-0.63)	281,342
Panel F: Additional controls and fixed effects						
Inventor experience	-0.060	(-5.93)	939,495	-0.050	(-2.20)	1,207,482
Team group characteristics	-0.064	(-5.25)	814,321	-0.011	(-2.42)	1,003,694
Team experience	-0.064	(-5.25)	814,321	-0.012	(-2.44)	1,003,694
Team fixed effects	-0.053	(-4.38)	700,219	-0.038	(-3.13)	979,288

TABLE 6

Effect of In-group Biases on Patenting Outcomes

This table regresses outcomes during the patent examination process on racial and gender mismatches between examiner and first inventor in the application. Panel A and B report results with racial and gender mismatches respectively. In columns (4), (5), and (6), the analysis is conducted for the sub-sample of applications that resulted in successful patent grants. First round grant is an indicator variable equal to one if an application resulted in a successful patent grant without receiving a non-final rejection, and zero otherwise. Appealed is an indicator variable equal to one if the applicant successfully requested a re-examination of the patent application and zero otherwise. Decision time refers to the number of calendar days from the date of initial application to the final decision or abandonment date. Δ number of claims is the change in number of claims allowed in a patent (usually a reduction) during the process of patent examination, expressed as a percentage of the initial number of claims. Δ words in claims is the change in number of words in the patent during the process of patent examination, expressed as a percentage of the initial number of words in the claims. Patent scope is the number of words in the first independent claim of a patent normalized by other patents within the same art unit, as measured by Kuhn and Thompson (2018). Racial (gender) mismatch is an indicator variable equal to one if the race (gender) of the first inventor on the application is not the same as that of the patent examiner, and zero otherwise. All variables are defined in Appendix A.1. t-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the art unit and application year level.

Panel A: Matches by Race

	First round grant	**Appealed**	**Decision time**	**Δ number of claims**	**Δ words in claims**	**Patent scope**
	(1)	(2)	(3)	(4)	(5)	(6)
Racial mismatch	-0.005	-0.001	2.790	-0.080	0.104	-0.012
	(-4.46)	(-1.89)	(2.46)	(-2.16)	(3.39)	(-1.68)
Foreign priority	-0.003	-0.005	-2.508	2.797	-0.062	-0.022
	(-1.20)	(-1.61)	(-0.68)	(2.34)	(-0.11)	(-1.80)
Foreign applicatin	-0.004	-0.001	-0.995	2.980	1.121	-0.061
	(-1.77)	(-0.50)	(-0.58)	(2.25)	(2.21)	(-4.97)
Small entity	0.005	-0.053	-46.795	0.666	-0.585	-0.151
	(1.91)	(-12.67)	(-18.28)	(0.60)	(-1.04)	(-6.99)
Examiner experience	-0.036	0.147	201.685	15.943	-0.015	-0.036
	(-12.76)	(11.12)	(15.43)	(10.05)	(-0.06)	(-2.37)
Art unit × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Examiner FE	Yes	Yes	Yes	Yes	Yes	Yes
Inventor FE	Yes	Yes	Yes	Yes	Yes	Yes
N	912,355	1,045,371	912,351	396,634	396,644	251,358
R^2	0.41	0.47	0.76	0.46	0.38	0.52

Panel B: Matches by Gender

	First round grant	**Appealed**	**Decision time**	**Δ number of claims (%)**	**Δ words in claims (%)**	**Patent scope**
	(1)	(2)	(3)	(4)	(5)	(6)
Gender mismatch	-0.003	-0.003	1.640	-0.610	0.308	-0.005
	(-1.72)	(-2.38)	(1.65)	(-1.85)	(1.90)	(-1.81)
Foreign priority	0.001	-0.007	-3.179	1.813	0.027	-0.018
	(0.56)	(-1.97)	(-0.82)	(2.43)	(0.06)	(-2.25)
Foreign application	-0.003	-0.003	-3.132	3.154	1.256	-0.077
	(-1.53)	(-1.41)	(-2.17)	(2.50)	(3.79)	(-6.92)
Small entity	0.007	-0.053	-47.529	0.278	-0.469	-0.149
	(2.82)	(-14.50)	(-20.03)	(0.35)	(-0.83)	(-7.18)
Examiner experience	-0.036	0.147	203.197	15.653	0.056	-0.030
	(-12.91)	(10.88)	(15.24)	(10.58)	(0.24)	(-2.21)
Art unit × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Examiner FE	Yes	Yes	Yes	Yes	Yes	Yes
Inventor FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1,174,256	1,345,697	1,174,246	508,382	508,398	321,956
R^2	0.54	0.56	0.68	0.49	0.41	0.40

TABLE 7
Racial Salience, Examiner Workloads, and In-group Biases

This table regresses patent grant decisions on racial mismatch as well as interactions with measures of salience of race and examiner workload. The dependent variable is an indicator variable equal to one if the patent application resulted in a final grant decision and zero otherwise. Racial mismatch is an indicator variable equal to one if the race of the first inventor on the application is not the same as that of the patent examiner, and zero otherwise. The variable racial conflict is equal to one when the negative media mentions of China are above median as compared to the sample period and zero otherwise. A mention is classified as negative, when the term China appears with a negative term in the headlines of news articles in the newspapers *Wall Street Journal*, *New York Times*, *Los Angeles Times* and *New York Post* with negative terms being obtained from the Harvard IV TagNeg dictionary. Segregation state is an indicator variable equal to one if the examiner received her earliest education at a university based in a state which enacted racial segregation laws, and zero otherwise. Examiner workload is an indicator variable equal to one if number of applications due with the patent examiner is above the median of her art unit in the given year, and zero otherwise. All control variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the art unit and application year level.

	Interaction variable		
	Racial conflict	Segregation state	Examiner workload
	(1)	(2)	(3)
Racial mismatch × interaction variable	-0.035	-0.020	0.011
	(-4.91)	(-3.75)	(0.55)
Racial mismatch	-0.038	-0.050	-0.050
	(-3.85)	(-3.51)	(-3.86)
Interaction variable	-0.990	-0.038	-0.023
	(-0.24)	(-4.91)	(-3.05)
Foreign priority	-0.036	-0.028	-0.018
	(-8.20)	(-5.57)	(-3.65)
Foreign application	-0.010	-0.025	-0.012
	(-6.37)	(-6.51)	(-5.16)
Small entity	-0.108	-0.145	-0.129
	(-34.86)	(-39.30)	(-16.32)
Examiner experience	0.114	0.127	0.100
	(24.55)	(25.43)	(25.11)
Art unit × year FE	Yes	Yes	Yes
Examiner FE	Yes	No	Yes
Inventor FE	Yes	Yes	Yes
N	939,495	859,324	911,862
R^2	0.55	0.56	0.50

TABLE 8

Examiner In-group Biases and Patent Quality

This table presents the effects of racial and gender mismatches between the examiner and the first-ranked inventor on measures of patent quality. Panels A and B report the results for race and gender mismatches respectively. Forward citations refer to the number of patents which have cited the given patent in the five year period after it has been issued. Backward citations is defined as the number of patents cited by the given patent, including self-citations. Generality is defined as one minus the Herfindahl-Hirschman Index (HHI) of the forward citations to the given patent. Originality is defined as one minus the HHI of backward citations by the given patent. In computing Generality and Originality, the HHI is calculated over the four-digit International Patent Classification (IPC) technology classes. Racial (gender) mismatch is an indicator variable equal to one if the race (gender) of the first inventor on the application is not the same as that of the patent examiner, and zero otherwise. All variables are defined in Appendix A.1. t-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the art unit and application year level.

Panel A: Matches by Race

	Forward Citations	**Backward Citations**	**Generality**	**Originality**
	(1)	(2)	(3)	(4)
Racial mismatch	0.093	1.813	0.070	0.002
	(2.58)	(15.94)	(2.78)	(3.45)
Foreign application	-0.304	0.328	-0.014	-0.001
	(-6.39)	(0.80)	(-0.19)	(-0.15)
Small entity	-0.148	-1.093	0.111	-0.002
	(-2.89)	(-1.44)	(0.95)	(-0.14)
Examiner experience	-0.207	-0.463	-0.022	0.012
	(-3.58)	(-1.40)	(-0.29)	(1.37)
Foreign priority	-0.076	-0.921	-0.020	0.005
	(-1.69)	(-1.75)	(-0.26)	(0.43)
Art unit × year FE	Yes	Yes	Yes	Yes
Examiner FE	Yes	Yes	Yes	Yes
Inventor FE	Yes	Yes	Yes	Yes
N	639,155	939,495	639,155	939,495
R^2	0.52	0.72	0.79	0.63

Panel B: Matches by Gender

	Forward Citations	**Backward Citations**	**Generality**	**Originality**
	(1)	(2)	(3)	(4)
Gender mismatch	0.048	0.036	0.019	0.002
	(2.26)	(6.17)	(1.27)	(4.37)
Foreign application	-0.299	0.113	-0.018	0.003
	(-7.25)	(0.35)	(-0.30)	(0.49)
Small entity	-0.118	-0.957	-0.053	-0.008
	(-2.64)	(-1.61)	(-0.37)	(-0.69)
Examiner experience	-0.229	-0.410	0.011	0.010
	(-4.87)	(-1.37)	(0.16)	(1.22)
Foreign priority	-0.047	-0.890	-0.007	0.005
	(-1.21)	(-2.26)	(-0.10)	(0.46)
Art unit × year FE	Yes	Yes	Yes	Yes
Examiner FE	Yes	Yes	Yes	Yes
Inventor FE	Yes	Yes	Yes	Yes
N	909,477	1,207,482	909,477	1,207,482
R^2	0.51	0.70	0.78	0.62

TABLE 9

Examiner In-Group Biases and Startup Outcomes

This table presents the effect of group mismatch between the examiner and first inventor on the application, on outcomes related to startups. Panels A and B report results from mismatches by race and gender respectively. In column (1), the dependent variable is an indicator variable equal to one if the first inventor forms a new startup firm in five years after the decision on her first patent application. In columns (2) to (6), the dependent variable is an indicator variable equal to one if the startup raises venture capital funding in years 1 to 5 after the decision on its first patent application. In column (7), the dependent variable is an indicator variable equal to one if the startup goes public after the decision on its first patent application. Racial (gender) mismatch is an indicator variable equal to one if the race (gender) of the first inventor on the application is not the same as that of the patent examiner, and zero otherwise. All variables are defined in Appendix A.1. t-statistics, reported in parentheses, are based on standard errors that that allow for double-clustering at the art unit and application year level.

Panel A: Matches by Race

	Startup formation	VC funding in 1 year	VC funding in 2 years	VC funding in 3 years	VC funding in 4 years	VC funding in 5 years	IPO
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Racial mismatch	-0.011	-0.008	-0.013	-0.013	-0.016	-0.020	-0.006
	(-2.05)	(-1.84)	(-1.71)	(-1.99)	(-1.67)	(-1.79)	(-3.15)
Foreign priority	0.014	-0.106	-1.013	-1.002	-1.072	-0.732	1.178
	(0.57)	(-7.10)	(-5.06)	(-4.77)	(-4.40)	(-4.59)	(6.65)
Foreign application	0.047	-0.103	-0.000	-0.002	-0.687	-0.290	0.798
	(2.20)	(-6.60)	(-5.14)	(-5.95)	(-4.54)	(-5.22)	(3.50)
Small entity	0.178	-0.338	-0.005	-0.013	-0.269	-0.178	4.390
	(4.85)	(-14.78)	(-3.10)	(-4.23)	(-5.20)	(-3.77)	(11.38)
Examiner experience	0.069	0.102	0.040	0.029	0.525	0.021	33.543
	(2.13)	(2.50)	(13.26)	(10.28)	(4.75)	(2.33)	(16.65)
Art unit × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Examiner FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	38,331	38,331	38,331	38,331	38,331	38,331	38,331
R^2	0.17	0.49	0.46	0.44	0.50	0.59	0.79

Panel B: Matches by Gender

	Startup formation	VC funding in 1 year	VC funding in 2 years	VC funding in 3 years	VC funding in 4 years	VC funding in 5 years	IPO
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender mismatch	-0.33	-0.009	-0.009	-0.014	-0.017	-0.021	-0.005
	(-4.50)	(-1.85)	(-1.85)	(-1.67)	(-1.60)	(-1.12)	(-2.46)
Foreign priority	-0.060	-0.277	-0.009	-0.003	-1.149	-1.144	-0.281
	(-1.01)	(-3.08)	(-1.64)	(-0.68)	(-4.42)	(-5.00)	(-0.17)
Foreign application	-0.175	-0.050	-0.009	-0.002	-0.621	-0.279	-0.453
	(-4.17)	(-2.31)	(-1.99)	(-0.89)	(-4.17)	(-6.04)	(-0.32)
Small entity	0.278	-0.420	-0.013	-0.006	-0.250	-0.061	2.940
	(10.35)	(-7.50)	(-6.52)	(-2.32)	(-4.30)	(-1.94)	(3.84)
Examiner experience	-0.591	0.152	0.002	0.036	1.096	0.110	21.351
	(-8.60)	(4.11)	(0.38)	(11.91)	(15.95)	(4.99)	(7.19)
Art unit × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Examiner FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	40,542	40,542	40,542	40,542	40,542	40,542	40,542
R^2	0.13	0.30	0.38	0.43	0.40	0.53	0.68

Appendix 1.A Variable Descriptions

TABLE A.1

Variable Definitions and Sources

This table defines the main variables used in the empirical analysis.

Variable name	Description	Source
Dependent variables		
Patent grant	An indicator variable equal to one if an application resulted in a successful patent grant and zero otherwise.	PatEx
First round grant	An indicator variable equal to one if an application resulted in a successful patent grant without receiving a non-final rejection and zero otherwise.	PatEx
Appealed	An indicator variable equal to one if the applicant successfully requested a re-examination of the patent application and zero otherwise.	PatEx
Δ number of claims	The change in number of claims allowed in a patent (usually a reduction) during the process of patent examination, expressed as a percentage of the initial number of claims.	Patent Claims Research Dataset
Δ words in claims	The change in number of words in the patent during the process of patent examination, expressed as a percentage of the initial number of words in the claims.	Patent Claims Research Dataset
Patent scope	The number of words in the first independent claim of a patent normalized by other patents within the same art unit, as measured by Kuhn and Thompson (2018).	Kuhn and Thompson (2018)
Key independent variables		
Racial mismatch	An indicator variable equal to one if the examiner and first-ranked inventor on the application do not belong to the same racial group, and zero otherwise.	PatEx, Tzioumis (2018), United States Census Bureau
Gender mismatch	An indicator variable equal to one if the examiner and first-ranked inventor on the application do not belong to the same gender, and zero otherwise.	PatEx, WIPO, Social Security Administration

Continued

TABLE A.1

Continued

Variable name	Description	Source
Control variables		
Decision time	Number of calendar days from the initial application date to the final decision made by the patent examiner.	PatEx
Foreign application	An indicator variable equal to one if the application is made by an inventor whose correspondence address is based in a country other than the United States, and zero otherwise.	PatEx
Small entity	An indicator variable equal to one if the applicant has been classified as a small entity by the USPTO, and zero otherwise.	PatEx
Examiner experience	The natural logarithm of the number of patent grant decisions made by the examiner prior to the application under consideration.	PatEx
Foreign priority	An indicator variable equal to one if the patent has been previously issued in a foreign jurisdiction, and zero otherwise.	PatEx
Racial salience and examiner workload		
Racial conflict	An indicator variable equal to one when the negative media mentions of China are above median as compared to the sample period and zero otherwise. A mention is classified as negative, when the term China appears with a negative term in the headlines of news articles in the newspapers *Wall Street Journal*, *New York Times*, *Los Angeles Times* and *New York Post* with negative terms being obtained from the Harvard IV TagNeg dictionary.	NewsBank Access World News Research Collection, Harvard IV TagNeg
Segregation state	An indicator variable equal to one if the examiner received her earliest education at a university based in a state which enacted racial segregation laws, and zero otherwise.	PatEx, LinkedIn
Examiner workload	An indicator variable equal to one if number of applications due with the patent examiner is above the median of her art unit in the application year, and zero otherwise.	PatEx
Patent quality		
Forward citations	The number of patents which have cited the given patent in the five year period after it has been issued.	PatentViews
Backward citations	The number of patents cited by the given patent, including self-citations.	PatentViews

Continued

TABLE A.1

Continued

Variable name	Description	Source
Generality	One minus the Herfindahl-Hirschman Index (HHI) of the forward citations to the given patent, where the HHI is calculated over the four-digit International Patent Classification (IPC) classes.	PatentViews
Originality	One minus the Herfindahl-Hirschman Index (HHI) of backward citations by the given patent, where the HHI is calculated over the four-digit International Patent Classification (IPC) classes.	PatentViews
Costs		
Startup formation	An indicator variable if the first inventor forms a new startup firm in the five years after the decision on her first patent application.	Thomson One VentureXpert
Venture Capital Funding	An indicator variable equal to one if the startup raises venture capital funding in years 1 to 5 after the decision on its first patent application.	Thomson One VentureXpert
IPO	An indicator variable equal to one if the startup goes public through an Initial Public Offering after the decision on its first patent application.	Thomson One VentureXpert, Thomson Reuter's Securities Data Company (SDC) database

Appendix 1.B Matching Procedure

I assign inventors and examiners to their race using a dictionary-matching process. I rely on two main race disambiguation datasets - the data on racial frequencies by last names in the 2000 and 2010 Census Surname Tables provided by the United States Census Bureau and the corresponding data on first names from Tzioumis (2018).[8]

The Census Surname Table reports last name frequencies by racial category based on responses to the decennial Census. I use these frequencies to compute the conditional probability that a given name belongs to a race. For example, the last name “Smith” appears in 2,442,977 observations in the 2010 Census data with 70.9% of respondents with this surname being Non-Hispanic Whites, 23.11% being Non-Hispanic Blacks, and the rest belonging to other racial categories. Accordingly, this last name is assigned 0.71 and 0.23 probabilities of belonging to the “white” and “black” racial groups respectively. I match the data between 2001 and 2009 to the 2000 Census Surname Table and the names recorded thereafter to the 2010 Table.

Overall, the census contains six racial categories - Non-Hispanic Whites, Hispanics, Non-Hispanic Black, Asian and Native Hawaiian and Other Pacific Islander, American Indian and Alaskan Native, and Two or More Races. Tzioumis (2018), while following similar methodologies and using the same racial categories, reports first name frequencies based on mortgage loan applications.

An inventor (examiner) is assigned to a race if the probability of both the first and the last name belonging to that racial category is greater than 0.70. I also apply stricter benchmarks of 0.75 and 0.80, and do not observe any significant impact on the results presented throughout this paper. A final concern might be that these two datasets mainly reflect names in the United States. To resolve these concerns, I assign unmatched names to their race using NamePrism API - a web-based algorithm.

Overall, this procedure assigns a race to 13,215 out of 16,127 examiners and 2,516,347 out of 2,814,389 inventors with non-missing names. This amounts to a 81.94% and 89.41% match for examiners and inventors respectively.

I identify the gender of the inventor and the examiner using state-level data on name frequency from the Social Security Administration (SSA). I first match the inventor to the state in which she resides, and the examiner to the state in which she received her earliest educational degree. A name is assigned to a gender when the percentage of names in the state belonging to that gender exceeds 70%. For individuals based outside the United States, I use the cross-country data provided by the World Intellectual Property Organization (WIPO) (Lax Martínez et al., 2016). Using this procedure, I match 87.88% (14,173 out of 16,127) of examiners and 92.58% (2,605,635 out of 2,814,389) of inventors to their gender.

[8] Detailed methodologies used to produce these datasets can be found in Comenetz (2016) and Tzioumis (2018) respectively.

Appendix 1.C Example of a Patent Application Form

PTO/AIA/14 (03-13)
Approved for use through 01/31/2014. OMB 0651-0032
U.S. Patent and Trademark Office; U.S. DEPARTMENT OF COMMERCE
Under the Paperwork Reduction Act of 1995, no persons are required to respond to a collection of information unless it contains a valid OMB control number.

Application Data Sheet 37 CFR 1.76	Attorney Docket Number	
	Application Number	
Title of Invention		

The application data sheet is part of the provisional or nonprovisional application for which it is being submitted. The following form contains the bibliographic data arranged in a format specified by the United States Patent and Trademark Office as outlined in 37 CFR 1.76.
This document may be completed electronically and submitted to the Office in electronic format using the Electronic Filing System (EFS) or the document may be printed and included in a paper filed application.

Secrecy Order 37 CFR 5.2

☐ Portions or all of the application associated with this Application Data Sheet may fall under a Secrecy Order pursuant to 37 CFR 5.2 (Paper filers only. Applications that fall under Secrecy Order may not be filed electronically.)

Inventor Information:

Inventor [Remove]

Legal Name

Prefix	**Given Name**	**Middle Name**	**Family Name**	**Suffix**

Residence Information (Select One) ◉ US Residency ◯ Non US Residency ◯ Active US Military Service

City		**State/Province**		**Country of Residence** i	
City		Country of Residence i			

Mailing Address of Inventor:

Address 1			
Address 2			
City		**State/Province**	
Postal Code		**Country** i	

All Inventors Must Be Listed - Additional Inventor Information blocks may be generated within this form by selecting the **Add** button. [Add]

Correspondence Information:

Enter either Customer Number or complete the Correspondence Information section below.
For further information see 37 CFR 1.33(a).

☐ **An Address is being provided for the correspondence Information of this application.**

Customer Number			
Name 1		**Name 2**	
Address 1			
Address 2			
City		**State/Province**	
Country i		**Postal Code**	
Phone Number		**Fax Number**	
Email Address		[Add Email]	[Remove Email]

EFS Web 2.2.8

Chapter 2

Attention-Induced Information Dry-Ups

2.1 Introduction

Institutional investors dominate today's financial markets. As of 2016, they hold more than 75% of the aggregate market value of all NYSE/AMEX/NASDAQ stocks. By allocating capital to firms and by monitoring firm management, they play a crucial role in the economy. Possibly due to the sheer size of their assets under management, the academic literature has long implicitly considered institutional investors to be unconstrained when it comes to the resources and attention they can devote to studying and engaging with firms in their portfolios.

Recent research challenges this view. Kempf et al. (2016) argue that institutional investors are constrained in their ability to monitor, and document that their lack of attention increases agency problems at the neglected firms. These agency problems can take a variety of forms, ranging from more value-destroying investment decisions and opportunistic executive compensation (Kempf et al., 2016), to less effective board monitoring (Liu et al., 2017) and more earnings management (Garel et al., 2018).

In this paper I argue that limited attention of institutional investors has important implications beyond investment and governance. I provide evidence consistent with the idea that limited attention of institutional investors has first-order effects on the information provided to investors in financial markets. I hypothesize that shifts in the demand for information by institutions, which occur because institutions try to optimize on where they allocate their limited attention, induces predictable shifts in the supply of information by financial analysts. As detailed below, the link between analyst effort and institutional attention could be direct, such that analysts observe and respond to shifts in institutional attention, or indirect, such that analyst behavior is a response to changed firm behavior due to limited monitoring by institutional investors. In either case, if institutions with limited attention focus on a given set of stocks, then analyst effort will also shift towards the same stocks. As a result, managers of firms that are not in the spotlight face lower scrutiny from both institutional investors and financial analysts at the same time, leading to attention-induced "information dry-ups."

Showing that institutional investor attention matters for financial analysts would have potentially important implications for how we think about the increasingly central role of institutional investors in financial markets, as well as about information provision in the financial analyst industry. Yet, establishing such a link empirically is inherently difficult. One of the central challenges is that attention cannot be directly observed. I overcome this challenge by using a new proxy for institutional investor attention, proposed by Kempf et al. (2016) (KMS), which exploits extreme returns in unrelated parts of institutional investors' portfolios and how exposed investors are to these shocks. Institutional investors who are temporarily reducing the attention they pay to a given firm are labeled "distracted." An important advantage of this measure is that the variation in attention originates from events in unrelated industries that are plausibly exogenous to the firm itself. This paper is thus able to measure the response of financial analysts to exogenous shifts in the demand for information by institutional investors.

From a conceptual standpoint, whether and how limited attention of institutions matters for effort provision by financial analysts is an empirical matter. Three broad scenarios are conceivable. First, shifts in institutional investor attention may not matter for the incentives of financial analysts. This could be because analysts are impartial observers of the activities in the firms they cover, and their rewards for being accurate are not very sensitive to the attention of institutional investors. A second possibility is that institutional shareholder distraction provides opportunities for financial analysts to add value, because KMS show that shareholder distraction leads to reduced monitoring by institutions and, in turn, to an increased tendency of managers to pursue private benefits. Thus, there might be an increase in analyst effort. The findings in this paper provide evidence consistent with a third scenario. Because institutional investors influence the prestigious *All-Star* analyst rankings (Hong et al., 2000), analyst bonuses (Groysberg et al., 2011), and promotions (Cen et al., 2017), analysts may decrease effort when institutional shareholders are distracted. As a result, analyst research may act as a complement to monitoring by institutional shareholders.

Using the KMS distraction measure on a dataset with more than 11,000 unique firms over the period 1990 to 2016, I find that forecast quality deteriorates after periods of high institutional investor distraction. Average forecast error increases by 9% for a one-standard-deviation increase in investor distraction. Consistent with the existing literature, which argues that less competition is usually associated with greater positive bias due to the internal incentive structure of sell-side research departments (Hong et al., 2000; Hong & Kacperczyk, 2010), I also document a substantial increase in forecast bias following investor distraction. These results hold both at the firm-quarter level, i.e., averaging across all analysts covering the firm, as well as at the analyst-firm-quarter level, where I can control for analyst $\times$ firm fixed effects.

My identification strategy removes important sources of potential confounding variation. First, by exploiting differences in the exposure of institutional shareholders to extreme returns in unrelated industries, the measure of attention used in this paper is plausibly exogenous to the performance of the firm itself. Second, industry $\times$ quarter fixed effects implicitly controls for any variable that does not vary across firms within a given industry and quarter, such as industry-wide investment opportunities, the state of the business cycle, etc. In addition, the results are also robust to including firm fixed effects. Hence, any firm-level time-invariant unobservable factor that might influence the match between a firm and its investors cannot impact my findings. Finally, because there are multiple forecasts by the same analyst at a given point in time, I can address concerns about heterogeneity in analyst skill, or other analyst-level attributes, via analyst × firm fixed effects. In other words, I can show that the forecasts of the *same* analyst covering the *same* firm are less accurate and more optimistic when the institutional shareholders of the firm are inattentive. This makes it possible to isolate the causal effect of investor distraction on analyst forecast quality.

The results on forecast accuracy and bias are of key importance to address concern about information provision in financial markets. They show that limited shareholder attention indeed affects the quality of the information produced by financial analysts, and thus, in turn, the information environment for individual firms. While this evidence is important, the forecast error and bias results have little power to discriminate between various drivers that could induce the observed link between institutional attention and forecast quality. Such drivers include reduced analyst effort, increased opportunistic managerial behavior induced by the lack of institutional monitoring (KMS), or changes in the information firms disclose when shareholders are distracted (e.g., Basu et al. (2017), Abramova et al. (2017)). All of these factors could potentially lead to lower-quality forecasts.

In order to investigate whether the decrease in forecast accuracy could be driven by reduced analyst effort, I test the relationship between investor inattention and observable measures of analyst effort. I find that analyst effort decreases after periods of investor inattention, both on the extensive margin and on the intensive margin. On the intensive margin, investor distraction reduces the number of forecasts issued per analyst, increases forecast delays, and reduces the length of questions asked in conference calls. On the extensive margin, the number of analysts covering the firm decreases by 3.8% for a one-standard-deviation increase in investor distraction. This is consistent with analysts having weaker incentives to spend time covering a given firm when its institutional shareholders are distracted.

These results cast doubt, for example, on any theory under which analysts are "fooled" by managers into thinking the firm is better than it actually is, because analysts who do not expect managers to be engaging in potentially hard-to-observe private benefit extraction. Likewise, less informative disclosure provided by firms alone would predict lower accuracy, but not necessarily less effort by analysts, unless analysts anticipate the higher marginal cost of analyzing the information and reallocate their effort accordingly. Hence, these results on analyst effort are consistent with analysts *strategically* allocating effort in response to changes in shareholder attention. This impact could either be direct, because analysts have an incentive to focus on the set of stocks their largest clients focus on, or indirect, because analysts know that shareholder distraction elicits a managerial response which raises the marginal costs of providing a high-quality forecast. In both cases, analysts trade off marginal benefits and marginal costs of providing a forecast for a given firm, and then allocate their effort accordingly across the stocks in their coverage portfolio.

I perform a variety of additional tests to support the interpretation that analysts reallocate effort across the stocks they cover in response to shareholder distraction. Most importantly, I study how an analyst's forecast accuracy in a given firm is affected by investor distraction in *other* firms covered by the *same* analyst at the *same point in time*. If analysts strategically reallocate their effort in response to institutional investor attention (either directly, or indirectly), then distraction in other firms in an analyst's portfolio should have a positive effect on accuracy. I find evidence consistent with this prediction: analysts with more distracted shareholders in the remainder of their portfolio are more accurate, and have less positive bias. This result strongly suggests that analysts allocate their effort away from firms held by distracted investors and towards firms held by investors who are paying attention, and that this shift in attention has a significant effect on their forecast accuracy.

These results are potentially important for our understanding of the role of institutional investors and sell-side research in shaping the information provided by financial markets. First, to the best of my knowledge, this is the first paper to causally identify the importance of institutional investors for the quality of sell-side research. Specifically, these findings imply that institutional investor attention provides a strong incentive for sell-side analysts to exert effort and provide accurate forecasts. Second, this paper contributes to our understanding of how analysts allocate their effort across firms in their portfolios, suggesting that a common proxy for the quality of the firm's information environment, the number of analysts following the firm, may be improved (see also Harford et al. (2018)). Finally, because the lack of monitoring by shareholders and directors coincides with a deterioration in the information environment of the firm, these results can help explain the magnitude of the agency problems associated with institutional investor distraction documented by prior studies. They imply an equilibrium in which it is optimal for institutional investors to pay attention to other firms, for financial analysts to shift effort towards the same firms, and for managers of neglected firms to shirk.

2.2 Data and Empirical Strategy

2.2.1 Shareholder Distraction Measure

To fix ideas, I begin by outlining the empirical predictions of this paper as well as detailing the shareholder distraction measure.

My starting point is an extension of the standard principal-agent framework (Berle & Means, 1932; Jensen & Meckling, 1976) to a model with three players: principals, agents, and financial analysts. In the absence of shareholder monitoring and external financial analysis, CEOs have an incentive to maximize private benefits, even if this reduces shareholder value. For example, the manager might make privately beneficial investments, such as a diversifying acquisition, or pay herself more. With shareholder monitoring or external financial analysis, the manager trades off private benefits with the cost of being caught. In general, greater monitoring intensity and more intensive financial analysis will induce CEOs to focus more on maximizing shareholder value.

I focus on monitoring by institutional investors, given the large theoretical literature motivating why and under which circumstances institutions can be effective monitors. Following

KMS, I assume that monitoring capacity is a scarce resource that can temporarily lead monitors to supply less than the otherwise optimal monitoring capacity. One way to think about the mechanism is to frame the monitor's problem as optimally allocating attention subject to a limited attention constraint, in the spirit of the rational inattention literature in economics (e.g., Sims (2003), Kacperczyk et al. (2016)). If institutional shareholders shift attention away from a firm, this loosens monitoring constraints and managers have greater leeway to maximize private benefits. KMS find empirical support for this prediction.

The main novelty in this paper is to consider the role of external financial analysts. Whether and how limited attention of institutions affects the effort provision by financial analysts depends on the structure of analysts' incentives. Three broad scenarios are conceivable. First, shifts in institutional investor attention may not matter for financial analysts if they do not affect their benefits from being accurate. A second possibility is that shareholder distraction provides an opportunity for financial analysts to add value, because KMS show that shareholder distraction leads to reduced monitoring by institutions and, in turn, to an increase tendency of managers to pursue private benefits. Thus, there might be an increase in analyst effort. Third, shareholder distraction may lead to a reduction in analyst effort. This could be because shareholders are less likely to read sell-side research on firms where they are distracted, or place a lower weight on analysts' accuracy on stocks to which they have not been paying attention when assigning All-Star status or when making hiring decisions.

Whether financial analysis acts as a substitute or complement to shareholder monitoring has first-order implications for the possibility of the CEO to extract private benefits. If it acts as a complement, then the possibility for CEOs to extract private benefits is limited, even when shareholders are distracted. If, on the other hand, it acts as a substitute, then the quality of analyst research will deteriorate simultaneously with shareholder monitoring, enabling the CEO to maximize private benefits.

I conjecture there exists an equilibrium in which it is optimal for institutional shareholders to pay attention to other firms, for financial analysts to shift effort towards the same firms, and for managers of neglected firms to shirk. I label such an equilibrium as an "information dry-up." The empirical findings of this study are consistent with the existence of such an equilibrium.x

My main variable of interest is a firm-level proxy for how much the "representative" institutional shareholder in a given firm f is distracted in a given period. I call this proxy *distraction*, and denote it by D. D is defined as in KMS. I focus on the attention of the institutional shareholders of the firm as opposed to the attention of the entire universe of institutional investors, because shareholders likely have a greater demand for sell-side equity research than non-shareholders.

The intuition behind D is straightforward: a given investor i in firm f is more likely distracted if there is an attention–grabbing event in another industry, and if that other industry is important in investor i's portfolio. I first compute an investor-level distraction score, and then aggregate across all investors in the firm. Specifically, I define D for each firm f and calendar quarter q as:

$$D_{fq} = \sum_{i \in F_{q-1}} \sum_{IND \neq IND_f} w_{ifq-1} \times w_{iq-1}^{IND} \times IS_q^{IND} \tag{2.1}$$

where F_{q-1} denotes the set of firm f's institutional shareholders at the end of quarter $q-1$, IND denotes a given Fama-French 12 industry, and IND_f denotes firm f's Fama-French industry. IS_q^{IND} captures whether a distracting event occurs in an industry other than IND_f, and w_{iq-1}^{IND} captures how much investor i cares about the other industry. The weight w_{ifq-1} captures how important investor i is for firm f.

KMS find that lower shareholder distraction is negatively correlated with conference call participation, changes in institutional investor portfolio, and shareholder proposals. Overall, the evidence suggests that 'distraction' plausibly measures institutional investor attention.

2.2.2 Data

I construct my main sample by merging several datasets. I begin by matching one- and two-quarter-ahead earnings forecasts from I/B/E/S from 1990 to 2016 with quarterly earnings announcements from the CRSP-Compustat merged database. As in prior literature, I only retain forecasts issued after 1990 due to concerns about insufficient coverage in I/B/E/S in preceding periods (Hong et al., 2000; Diether et al., 2002).

I apply standard filters from the literature to this merged dataset. First, I retain only the most recent forecast issued for a firm by an analyst in the quarter. Second, to exclude stale forecasts, I retain only forecasts issued or revised within 60 calendar days immediately preceding the earnings announcement date. Finally, to eliminate outliers, I drop observations where the unadjusted stock price is lower than \$5 or where the adjusted earnings or forecast is greater than the adjusted stock price.

I then merge this dataset with financial statement data from Compustat, with data on share prices, returns, trading volume, and bid-ask spreads from CRSP, and with institutional holding information from the Thomson Reuters 13f database. I exclude micro-caps, defined as stocks with market value below the 20th NYSE percentile breakpoint following Fama & French (2008), as they are not relevant for most institutional investors. The final sample consists of 359,979 firm-quarter observations and 11,073 unique firms. I also construct a more disaggregated version of the dataset at the analyst-firm-quarter level with 1,698,886 observations, 8,158 unique firms, and 14,815 analysts.

I study two aspects of analyst forecast quality, namely, forecast optimism and forecast accuracy. I measure forecast bias as the difference between the mean earnings per share forecast issued by the analysts covering the firm and its actual earnings in quarter *t+1*, scaled by the share price of the firm at the end of the previous quarter, as in Hong & Kacperczyk (2010). Forecast accuracy is defined as the absolute value of bias. Since institutional owners have been shown to value accurate earnings forecasts (Ljungqvist et al., 2007), these two measures capture a relevant dimension of the quality of the information available to the wider financial market.

I measure analyst effort both at the intensive and the extensive margin. For the intensive margin, I consider three distinct measures of analyst effort, defined as in Merkley et al. (2017).

First, I examine changes in the number of forecasts issued by the average analyst. This measure is defined as the total number of forecasts issued for a given firm in quarter *t+1*, divided by the number of analysts who issued at least one forecast for the firm in quarter *t-1*. Second, I measure the 'delay' with which these forecasts are issued. I expect that analysts issue forecasts earlier for firms to which they have allocated greater effort (Hong et al., 2000; Cooper et al., 2001). Delay is defined as the number of days between the end of quarter *t* and the earliest forecast in the subsequent quarter. Finally, I study the average length of the questions asked by analysts in conference calls. This measure permits direct analysis of the observable aspect of the effort exerted by analysts in interacting with managers of firms for which they are issuing forecasts. Taken together, these three measures gauge the amount of effort that analysts dedicate to covering the stocks in their portfolio.

I measure the extensive margin of analyst effort as the change in the number of active analysts covering a firm in a given quarter. Changes in the number of active analysts are defined as the difference between analyst entries and exits. Following Merkley et al. (2017), analyst entries are defined as the total number of analysts who have either issued a forecast in quarter *t+1*, after not having done so for the preceding four quarters, or who are issuing their first forecast for the firm in the sample in *t+1*. Similarly, analyst exits are measured as the number of analysts who issue a forecast in *t* but not in the next four quarters.

Table 1 provides the summary statistics for my main sample. As noted previously, institutional owners are an economically important group of shareholders, holding 38% of firms' shares on average. Additionally, of the shares held by institutional investors, 46% are held by the five largest investors in the firm. The average mean forecast error and bias are 0.65% and 0.27%, respectively, in line with previous studies. About 11 analysts issue a forecast for a firm in a given quarter, and a one-standard-deviation change in the number of analysts is equal to one analyst per quarter.

2.2.3 Empirical Strategy

The following thought experiment illustrates my empirical approach. Consider two otherwise identical firms 1 and 2 in a given industry and quarter. Firm 1's representative shareholder holds two stocks. The first is firm 1 itself, and the second is another firm belonging to a different industry, which for the sake of this example I call "banks". The representative shareholder of firm 2 does not hold any bank stocks. Suppose now that there is an attention-grabbing event in the banking industry; for example, a banking crisis that sends prices of bank stocks falling. Assuming limited attention, the representative shareholder in firm 1 may, potentially rationally, shift attention towards banks and away from firm 1. If sell-side analysts follow the attention of institutional investors, then the amount of effort they allocate to researching firm 1 decreases, and their forecast accuracy declines. In contrast, and by construction, firm 2 is not affected. I can therefore identify the impact of variation in investor attention on analyst effort and analyst forecast accuracy by analyzing changes in analysts' effort and forecast accuracy on firm 1 relative to firm 2 following the exogenous shock. Following KMS, I use "extreme" industry returns (both positive and negative) as the main empirical proxy for attention-grabbing events.

I implement this idea by regressing changes in analyst coverage and analyst effort on lagged institutional investor distraction:

$$y_{fj,t+1} = \alpha_{j,t+1} + \alpha_f + \beta Distraction_{fjt} + \gamma' X_{fjt} + \epsilon_{fj,t+1}, \tag{2.2}$$

where the dependent variable, $y_{fj,t+1}$ refers to average analyst forecast accuracy or average analyst effort, respectively, in firm f operating in industry j in quarter *t+1*. $Distraction_{fjq}$ is the weighted average distraction of the institutional shareholders in firm f at time t, defined as in KMS and measured as a moving average during quarter t and the previous three calendar quarters. I relate changes in forecast quality and analyst effort to institutional shareholder distraction *in the previous four quarters* for two main reasons. First, by using lagged instead of contemporaneous changes in investor attention, I allow analysts to observe and react to changes in investor attention with some lag. The underlying assumption is that preparing earnings forecasts and talking to management may take time, and that the amount of time and effort analysts spend on researching the firm in the current quarter likely affects the quality of the forecasts made in the next quarter, even if investors are now less distracted. Second, by measuring institutional investor distraction over longer horizons, I am capturing the effect of prolonged periods of inattention, which are arguably more likely to induce a reaction in analysts' allocation of effort.[1]

The set of fixed effects included further mitigates concerns about potential confounding factors. $\alpha_{j,t+1}$ are industry $\times$ quarter fixed effects, so that I compare firms within the same industry at a given point in time, as in the motivating example above. This permits ruling out the effect of any factors that do not vary within industry-date. In particular, it implicitly controls for the possibility that some industries may be more related to the "shock" industry that experiences extreme returns, e.g., due to supplier relationships. In addition, by including firm fixed effects (α_f), I control for time-invariant unobserved heterogeneity. This ensures that selection stories in which some unobservable, time-invariant, variable matches firms that receive less analyst coverage with investors exposed to "shock" industries. Additional controls (X_{fjt}) include the level of institutional ownership in *t-1* and institutional ownership concentration as in Hartzell & Starks (2003), so these results are not subsumed by standard measures of institutional ownership structure. Following Hong & Kacperczyk (2010), I also control for the log of the firm's market capitalization, the quarterly stock return and stock return volatility in quarter t, the book-to-market-ratio, volatility of ROE, and profit. I provide a complete list of variable definitions in Appendix A.1.

[1] In the robustness tests, I also report results when distraction is measured over one quarter only. The results are qualitatively and quantitatively very similar.

2.3 Main Results

2.3.1 Shareholder Distraction and Analyst Forecast Quality

As discussed in Section 2.2.1, there might be larger analyst forecast errors when institutional investors are distracted and sell-side research acts as a complement to shareholder monitoring. In addition, if less analyst effort reduces competition, this might result in more optimistically biased forecasts (see Hong & Kacperczyk (2010), Merkley et al. (2017)).

Table 2 presents the results. Columns (1) and (2) show results for the mean forecast error, measured as the absolute difference between the consensus mean forecast and the actual earnings per share. Columns (3) and (4) report results for mean forecast bias as the dependent variable, defined as the difference between the consensus mean forecast and the actual earnings per share. Columns (1) and (3) include industry × quarter fixed effects, and columns (2) and (4) add firm fixed effects. Standard errors are clustered at the firm level in all regressions.

The results suggest that forecast accuracy deteriorates and forecast bias increases as institutional investor attention decreases. The coefficient on distraction in column (2) implies that a one-standard-deviation increase in shareholder distraction leads to an increase in the average forecast error of 8.7% ($=1.415 \times 0.04/0.65$). The effect on mean forecast bias, reported in columns (3) and (4), is similarly sizable. The results are stronger once firm fixed effects are included, suggesting that unobserved heterogeneity at the firm level is biasing against finding a significant effect. Importantly, the inclusion of firm fixed effects allows ruling out the possibility that the earnings of firms with distracted shareholders are *always* harder to forecast. Overall, these findings suggest that institutional shareholder distraction reduces the quality of sell-side analyst forecasts, consistent with the hypothesis that analysts' incentives to produce accurate forecasts are weaker when institutions are paying less attention.

I also ensure that these findings are robust to performing the analysis at the individual analyst level, where forecast error and forecast bias can be computed for an individual analyst covering a certain stock in a given quarter, thereby controlling for additional confounding factors. This approach also makes it possible to disentangle whether the higher forecast error is driven by changes in the composition of analysts or brokerage firms covering the firm, or by changes in forecast accuracy within the same analyst or within the same brokerage house for the same firm.

Table 3 reports the results. I estimate regression 2.2 at the analyst-firm-quarter level, and sequentially add higher-dimensional fixed effects. Columns (1) and (4) add broker fixed effects, columns (2) and (5) add analyst fixed effects, and columns (3) and (6) add analyst × firm fixed effects. The specification with analyst × firm fixed effects is particularly informative, because it implies that forecasts by the *same* analyst for the *same* firm are less accurate when distraction is high. The point estimate of the coefficient on shareholders distraction is hardly affected by including these additional fixed effects. This suggests that the increase in forecast bias and forecast error is largely driven by within-brokerage firm and within-analyst changes in accuracy for the same firm, as opposed to changes in the composition of analysts covering the firm (e.g., more accurate analysts dropping coverage of the firm). It also substantially raises the bar for alternative explanations. The economic significance is even larger in this more disaggregated sample: the estimates from the strictest specification (column (3)) imply a 14.8% (=1.829 × 0.05/0.62) increase in forecast error for a one-standard-deviation increase in distraction.

I perform additional robustness tests for the results on forecast error in Table 4. First, the results are effectively unchanged upon estimating with the median instead of the mean forecast error (Panel A). In Panel B, I use distraction measured in quarter t as opposed to the moving average measured over quarter *t-3* to t. The results are qualitatively and quantitatively very similar, suggesting that even a relatively short period of investor distraction such as one quarter can lead to a substantial decline in analyst forecast quality. Panel C changes the timing of the control variables to *t-1* instead of t, in order to address the possibility that these variables may also be outcomes of investor distraction and, hence, create a problem of endogenous controls. Next, I address the potential issue that, within a given Fama-French industry, some firms may be mechanically related to the shock industry because they are misclassified, and may be harder to forecast because they are directly affected by the new information released in the shock industry. I define an industry relatedness variable as follows: I first obtain, for each firm, the set of closely related firms from the Hoberg & Phillips (2010) text-based industry classification dataset from Professor Gerard Hoberg's website; I then compute, for each firm, the percentage of related firms which operate in the shock industries to obtain a firm-specific proxy for the severity of the misclassification problem. Because the Hoberg-Phillips data start only in 1996, I use the first available firm-pair in all previous periods in which I observe both firms in the data. The results in Panel C indicate that relatedness to the shock industries does not induce my previous results. In Panel D, I address concerns that data on institutional investor holdings from Thomson Reuters 13f after 2012 contains inaccuracies (Gilje et al., 2018), by dropping observations after 2012. Despite this change in sample size, I find that the results are stronger, both in terms of statistical significance and economic magnitude. Finally, in Panel E I estimate the regressions at the annual rather than at the quarterly frequency. The magnitude of the economic is, if anything, stronger than in the quarterly data, but statistical significance is lower.

Overall, the results presented so far strongly support a causal and robust effect of institutional investor distraction on the quality of earnings forecasts.

2.3.2 Shareholder Distraction and Analyst Effort

My results on forecast accuracy and forecast bias show that limited shareholder attention indeed affects the quality of the information produced in financial markets, and thus, in turn, the information environment for individual firms. Yet, they have little power to discriminate between various drivers that could induce the observed link between institutional attention and forecast quality. These include reduced analyst effort, increased opportunistic managerial behavior induced by the lack of institutional monitoring (KMS), or changes in the information firms disclose when shareholders are distracted (e.g., Basu et al. (2017), Abramova et al. (2017)). All of these factors could potentially lead to lower-quality forecasts.

In order to investigate whether part of the decrease in forecast accuracy could be driven by reduced analyst effort, I test whether institutional investor distraction is related to direct measures of analyst effort. I consider three proxies for analyst effort proposed in the literature. First, I study changes in the average number of forecasts issued per analyst, following Merkley et al. (2017). Second, I look at the average delay of analysts' forecasts, because Hong et al. (2000) and Cooper et al. (2001) argue that longer delays reflect lower analyst effort. Delay is computed as the number of days between the end of the previous quarter t and the earliest forecast issued by the analyst in the current quarter $t+1$. Finally, I measure analyst effort using the average length of the questions asked by analysts in conference calls (see Merkley et al. (2017)).

The estimated results are presented in Table 5. All three proxies for analyst effort indicate that analysts work less hard when institutional shareholders have been focusing their attention elsewhere. Analysts issue 3.2% ($= -0.453 \times 0.04/0.57$) fewer forecasts during a quarter following investor distraction (columns (1) and (2)), their forecasts have a one-day ($= 26.109 \times 0.04$) greater delay (columns (3) and (4)), and they ask firm managers shorter questions in conference calls (columns (5) and (6)). The latter effect is economically modest and amounts to ca. 0.4 ($=9.205 \times 0.04$) fewer words per question.

An extreme form of reducing effort would be to stop coverage of a firm all together. It is possible that brokerage firms which are on the margin of whether or not to cover a specific stock, or how many analysts to assign, may be more likely to remove an analyst after a period of prolonged investor inattention. To test for an effect of investor distraction on the extensive margin of analyst effort, I consider changes in the number of analysts following the firm. Following Merkley et al. (2017), changes in analyst coverage are defined as the difference between the number of analysts who do not issue any forecast for the firm in the next four quarters and the number of analysts who start to issue a forecast, after not having done so for the past four quarters. Hence, this measure of analyst coverage captures the effect of shareholder distraction on *persistent* changes in analyst coverage. Since the number of analysts following is a central component of a firm's information environment and has been shown to affect firms' financing and investment policies (e.g., O'Brien & Bhushan (1990), Hong & Kacperczyk (2010), Derrien & Kecskés (2013), Balakrishnan et al. (2014)), this is an important result.

Table 5, columns (7) and (8), reports the results. Periods of high investor distraction lead to a persistent decrease in the number of analysts following the firm. The point estimate in column (8) implies 0.04 ($= -0.986 \times 0.04$) fewer analysts, on average, for a one-standard-deviation increase in distraction. I find very similar results upon replacing changes in the number of analysts following by changes in the number of brokerage houses covering the firm (results unreported for brevity).

These results cast doubt on any theory under which analysts are deceived by managers into thinking that the firm is better than it actually is, and remain in the dark about managers engaging in potentially hard-to-observe private benefit extraction. The reason is that such a theory would not predict that analysts scale back effort or drop coverage. Likewise, less informative disclosure provided by firms alone would not lead to less effort, unless analysts observe the change in disclosure and then reallocate their effort in response. Hence, the above results support the view that sell-side analysts *strategically* re-optimize their effort in response to changes in institutional investor attention. Importantly, they also imply that sell-side analyst research and institutional shareholder monitoring act as complements rather than substitutes, thereby leading to substantial time-variation in the quality of the information environment of the firm.

2.3.3 Shareholder Distraction in Other Firms

So far the findings suggest that analysts reduce their effort in response to a decrease in institutional investors' attention on the firms they cover. A remaining question is whether analysts substitute the reduced effort on one stock for more leisure, or for additional effort on other stocks they cover, where shareholders are paying more attention. In the latter scenario, institutional investor distraction on *other* stocks that the analyst covers should lead to greater forecast accuracy. Specifically, if analysts optimally reallocate their effort across their portfolio based on institutional investor attention, then one would expect analysts with more distracted stocks in the rest of their portfolio to exert greater effort and exhibit greater accuracy, compared to other analysts with less distracted firms in the rest of their portfolio.

In order to test this prediction, I compute a variable *Other distraction*, which is defined as the value-weighted average shareholder distraction computed over all other firms covered by the analyst, excluding the firm itself. I then repeat the analysis in Table 3, while adding *Other distraction* to the regression equation. Table 6 presents the results. As predicted by my hypothesis, higher shareholder distraction on *other* firms in the analyst's portfolio increase her accuracy compared to other analysts with less shareholder distraction in the rest of their portfolio. Analyst forecasts for a firm become 2.4% ($= -0.291 \times 0.05/0.62$) more accurate and 5.6% ($= -0.303 \times 0.05/0.27$) less optimistic when shareholder distraction for other firms in their portfolios increases by one standard deviation.

In sum, analysts appear to strategically reallocate effort across the stocks in their portfolio, in line with institutional shareholder attention. This leads to substantial time-variation in the quality of the information environment of the firm.

2.4 Conclusion

I study the effect of institutional investor attention on the quality of sell-side analyst research. Using firm-level institutional shareholder "distraction" measures, I show that analyst forecast quality deteriorates when institutional shareholders focus their attention on unrelated industries. This loss in forecast accuracy coincides with a reduction in observable measures of analyst effort, such as the number of forecasts issued per analyst, average forecast delay, and the average length of questions asked by analysts in conference calls. Overall, these patterns are consistent with a model in which analyst coverage follows the attention of institutional investors, leading to periods of "information dry-up." In these periods, managers can get away with shirking, because the lack of monitoring by shareholders coincides with a deterioration in the information environment of the firm. My results can therefore help explain the magnitude of the agency problems associated with institutional investor distraction documented by prior studies.

TABLE 1
Summary Statistics

The table presents summary statistics for key variables. Panel A reports the descriptive statistics for the aggregate sample, which contains firm-quarter observations from 1990 to 2016. Panel B presents the statistics for the disaggregated sample with analyst-firm-quarter observations. A complete list of variable definitions is provided in Appendix A.1.

Panel A: Aggregated Sample (Firm-Quarter Level)

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent variables: Forecast quality						
Mean forecast error$_{t+1}$	199,971	0.65	1.15	0.07	0.21	0.61
Median forecast error$_{t+1}$	199,971	0.65	1.15	0.06	0.20	0.61
Mean forecast bias$_{t+1}$	199,971	0.27	1.02	-0.13	0.01	0.33
Median forecast bias$_{t+1}$	199,971	0.27	1.02	-0.13	0.00	0.32
Dependent variables: Analyst coverage and effort						
Forecasts per analysts$_{t+1}$	200,294	0.57	0.52	0.23	0.38	0.67
Delay$_{t+1}$	223,781	71.68	29.00	50.31	68.70	88.38
Question length$_{t+1}$	68,880	34.23	9.39	27.59	33.86	40.26
Number of analysts$_{t+1}$	210,297	10.89	6.65	5.00	9.00	15.00
Δ Analysts$_{t+1}$	210,297	0.01	1.04	-1.00	0.00	1.00
Dependent variables: Information quality						
Abnormal volume$_{t+1}$	354,216	-0.282	1.034	-0.771	-0.221	0.272
Abnormal volatility$_{t+1}$	382,024	-0.181	1.727	-1.099	0.011	0.955
Bid-Ask Spread$_{t+1}$	319,747	0.110	0.550	-0.379	0.068	0.197
Amihud$_{t+1}$	378,881	0.367	1.182	-0.984	0.345	0.466
Key independent variables						
Distraction	352,600	0.16	0.04	0.13	0.16	0.18
Control variables						
IO$_{t-1}$	329,087	0.38	0.29	0.08	0.37	0.62
Top 5 IO share$_{t-1}$	335,021	0.46	0.26	0.32	0.44	0.62
Log of market cap$_t$	352,055	6.72	1.37	5.75	6.74	7.80
Stock price return$_t$	352,537	0.01	0.06	-0.02	0.01	0.05
Return volatility$_t$	352,512	0.09	0.07	0.04	0.07	0.12
Log of B/M ratio$_t$	213,742	-0.81	0.70	-1.28	-0.77	-0.33
Volatility of ROE$_t$	213,837	0.03	0.04	0.01	0.01	0.03
Profit$_t$	215,722	0.02	0.03	0.01	0.02	0.04

Panel B: Disaggregated Sample (Analyst-Firm-Quarter Level)

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent variables: Forecast quality						
Forecast error$_{t+1}$	1,221,586	0.62	1.04	0.06	0.20	0.61
Forecast bias$_{t+1}$	1,221,586	0.27	0.94	-0.13	0.00	0.33
Key independent variables						
Distraction	1,665,404	0.15	0.05	0.12	0.15	0.18
Other distraction	1,665,298	0.13	0.05	0.10	0.13	0.16
Control variables						
IO$_{t-1}$	1,194,077	0.49	0.32	0.21	0.57	0.74
Top 5 IO share$_{t-1}$	1,226,394	0.32	0.2	0.24	0.34	0.43
Log of market cap$_t$	1,665,298	8.03	1.47	6.94	7.95	9.12
Stock price return$_t$	1,665,375	0.01	0.06	-0.03	0.01	0.05
Return volatility$_t$	1,665,365	0.09	0.06	0.05	0.08	0.12
Log of B/M ratio$_t$	1,627,742	-0.91	0.7	-1.37	-0.87	-0.41
Volatility of ROE$_t$	1,628,935	0.03	0.04	0.01	0.01	0.03
Profit$_t$	1,641,519	0.02	0.02	0.01	0.02	0.04
High$_t$	1,665,365	0.24	0.43	0.00	0.00	0.00
Low$_t$	1,665,365	0.21	0.41	0.00	0.00	0.00
General experience$_t$	1,665,365	24.22	16.97	10.00	20.00	35.00
Firm-specific experience$_t$	1,665,365	10.29	9.04	3.00	7.00	15.00
Forecast age$_t$	1,665,365	52.57	24.94	32.00	59.00	73.00
Portfolio size$_t$	1,665,365	12.97	6.39	8.00	12.00	17.00
SIC2$_t$	1,665,365	3.19	1.99	2.00	3.00	4.00
Brokerage resources$_t$	1,665,365	0.08	0.28	0.00	0.00	0.00

TABLE 2
Shareholder Distraction and Average Forecast Quality

This table regresses average next-period analyst forecast quality on institutional investor distraction. In specifications (1) and (2), the dependent variable is mean forecast error, which is the absolute value of the mean forecast bias, defined as the difference between the consensus (mean) forecast issued by analysts and the actual earnings per share announced in quarter *t+1*, scaled by the share price at the end of quarter *t*. In specifications (3) and (4), the dependent variable is the mean forecast bias. Forecast bias and error are defined as in Hong & Kacperczyk (2010). Institutional investor distraction is defined over quarters *t-3* to *t*, as in Kempf et al. (2016). Control variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are robust to clustering at the firm level.

	Mean forecast error$_{t+1}$		Mean forecast bias$_{t+1}$	
	(1)	(2)	(3)	(4)
Distraction	0.802	1.415	0.847	1.406
	(2.16)	(4.20)	(2.44)	(4.25)
IO$_{t-1}$	0.045	0.027	0.066	0.044
	(2.08)	(1.23)	(3.70)	(2.10)
Top 5 IO share$_{t-1}$	-0.014	-0.093	-0.163	-0.124
	(-0.45)	(-2.92)	(-5.81)	(-4.11)
Log of market cap$_t$	-0.017	-0.132	-0.003	0.037
	(-2.84)	(-12.81)	(-0.49)	(3.99)
Stock price return$_t$	-1.110	-0.605	-1.401	-0.868
	(-19.45)	(-11.09)	(-25.41)	(-16.17)
Return volatility$_t$	1.186	0.613	0.682	0.338
	(19.06)	(11.70)	(12.07)	(6.66)
Log of B/M ratio$_t$	0.476	0.499	0.235	0.367
	(46.15)	(43.33)	(27.94)	(32.75)
Volatility of ROE$_t$	9.770	8.776	5.486	5.295
	(55.85)	(62.52)	(42.42)	(41.90)
Profit$_t$	-3.575	-3.475	-3.050	-3.545
	(-17.22)	(-15.00)	(-14.83)	(-14.69)
Industry× quarter FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
N	182,150	181,836	182,150	181,836
R^2	0.29	0.40	0.15	0.27

TABLE 3

Shareholder Distraction and Forecast Quality – Analyst-Level

This table regresses next-period analyst forecast quality (at the analyst-firm level) on institutional investor distraction. In specifications (1) to (3), the dependent variable is the analyst forecast error, which is the absolute value of the forecast bias, defined as the difference between the forecast issued by the analyst and the actual earnings per share announced in quarter *t+1*, scaled by the share price at the end of quarter *t*. In specifications (4) to (6), the dependent variable is the analyst forecast bias. Forecast error and bias are defined as in defined as in Hong & Kacperczyk (2010). Institutional investor distraction is defined over quarters from *t-3* to *t*, as in Kempf et al. (2016). Control variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are robust to clustering at the firm level.

	Forecast error$_{t+1}$			Forecast bias$_{t+1}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	1.896	1.894	1.829	1.701	1.679	1.352
	(4.50)	(4.64)	(4.34)	(4.23)	(4.31)	(3.29)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry×quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Brokerage FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst FE	No	Yes	No	No	Yes	No
Analyst×firm FE	No	No	Yes	No	No	Yes
N	908,748	907,842	871,860	908,748	907,842	871,860
R^2	0.42	0.43	0.49	0.28	0.29	0.37

TABLE 4

Robustness

This table presents robustness checks. The baseline regression refers to columns (1) and (2) of Table 2. For brevity I only report coefficients of interest and suppress the control variables. In Panel A, median forecast error is defined as the absolute difference between the consensus (median) forecast issued by analysts and the actual earnings per share in quarter *t+1*, scaled by the share price at the end of quarter *t*. In Panel B, I measure distraction over one quarter only (quarter *t*), following Kempf et al. (2016). In Panel C, I estimate the baseline regression with additional controls. Lagged controls refers to a specification in which all controls are measured in quarter *t-1*. Industry relatedness is defined as the % of firms which operate in the shock industries out of the total sample of closely related firms, where the latter is defined as in Hoberg & Phillips (2010). In Panel D, I estimate the results using data before and including the year 2012, to alleviate concerns arising from inaccuracies in Thomson Reuters 13f after 2012 (Gilje et al., 2018). Finally, in Panel E, I report coefficients obtained using annual forecasts, i.e., with firm-year observations.

	OLS			Firm FE			
	Coeff.	*t*-stat	Econ. magnitude	Coeff.	*t*-stat	Econ. magnitude	N
Baseline	0.802	2.16	0.049	1.415	4.20	0.087	181,836
Panel A: Alternative measures of forecast quality							
Median forecast error	0.772	2.07	0.047	0.901	4.06	0.055	156,037
Panel B: Alternative measures of distraction							
One-quarter distraction	0.782	3.12	0.084	0.953	4.32	0.103	156,037
Panel C: Additional controls							
Lagged controls	0.968	2.42	0.060	1.504	4.07	0.092	175,850
Industry relatedness	1.337	2.83	0.082	1.276	2.81	0.079	122,213
Panel D: Sample size							
Sample truncated at 2012	1.736	4.38	0.109	1.610	4.28	0.101	149,182
Panel E: Estimation method							
Annual frequency	2.023	1.95	0.133	1.534	1.70	0.101	40,292

TABLE 5
Shareholder Distraction and Average Analyst Effort

This table regresses analyst effort on institutional investor distraction. In specifications (1) and (2), the dependent variable is the number of forecasts issued by active analysts in quarter *t+1* per analysts issuing forecasts in *t-1*. In columns (3) and (4), the dependent variable is the average number of days between the end of the quarter t in which distraction event occurs and the first forecast issued by an analyst in the subsequent quarter $t+1$. In columns (5) and (6), the dependent variable is the average length of questions asked by analysts in conference calls in the three quarters after distraction. Length is measured as the ratio of words spoken by analysts to the number of questions asked during the call, following Merkley et al. (2017). In columns (7) and (8), the dependent variable is defined as the change in number of analysts from quarter t to *t+1*. Institutional investor distraction is defined over quarters *t-3* to t as in Kempf et al. (2016). Control variables are defined in Appendix A.1. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

	Forecasts per analysts$_{t+1}$		Delay$_{t+1}$		Question length$_{t+1}$		Δ Analysts$_{t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distraction	-1.600	-0.453	50.719	26.109	-23.174	-9.205	-0.865	-0.986
	(-6.00)	(-2.11)	(4.68)	(2.41)	(-3.59)	(-1.92)	(-3.29)	(-3.17)
IO$_{t-1}$	-0.283	-0.207	-4.270	-0.750	0.712	-0.064	-0.026	-0.084
	(-16.00)	(-11.97)	(-5.70)	(-0.84)	(1.99)	(-0.21)	(-2.23)	(-4.40)
Top 5 IO share$_{t-1}$	0.456	0.332	3.068	1.033	-0.619	0.075	0.115	0.187
	(17.92)	(14.37)	(2.69)	(0.86)	(-1.04)	(0.15)	(5.88)	(6.87)
Log of market cap$_t$	-0.167	-0.126	0.652	0.398	2.248	0.606	0.029	0.062
	(-44.12)	(-23.94)	(3.95)	(1.31)	(16.33)	(3.11)	(10.72)	(8.49)
Stock price return$_t$	0.444	0.259	3.815	0.799	-6.839	-2.856	0.695	0.326
	(16.72)	(11.49)	(2.46)	(0.51)	(-8.85)	(-4.39)	(14.27)	(6.36)
Return volatility$_t$	-0.310	-0.023	5.559	4.349	6.062	0.663	0.186	0.081
	(-10.14)	(-1.09)	(3.55)	(3.06)	(5.82)	(1.03)	(4.30)	(1.67
Log of B/M ratio$_t$	-0.003	-0.047	2.280	0.195	-0.227	0.100	-0.111	-0.188
	(-0.42)	(-8.23)	(9.12)	(0.59)	(-1.31)	(0.50)	(-23.86)	(-21.13)
Volatility of ROE$_t$	-0.128	-0.025	22.305	13.166	7.771	0.593	-0.849	-0.795
	(-2.16)	(-0.57)	(8.05)	(5.00)	(4.31)	(0.41)	(-12.93)	(-9.21)
Profit$_t$	0.942	0.484	26.147	4.710	-9.385	-4.518	0.842	1.582
	(8.66)	(5.17)	(5.39)	(0.83)	(-2.62)	(-1.62)	(7.12)	(8.86)
Industry×quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes	No	Yes	No	Yes
N	147,199	146,836	146,274	145,896	60,143	60,053	182,459	182,139
R^2	0.23	0.52	0.19	0.31	0.32	0.59	0.09	0.13

TABLE 6
Shareholder Distraction in Other Firms and Forecast Quality

This table regresses next-period analyst forecast quality (measured at the analyst-firm level) on institutional investor distraction in the firm, as well investor distraction in other firms in the analyst's portfolio. In specifications (1) to (3), the dependent variable is analyst forecast error, which is the absolute value of forecast bias, defined as the difference between the forecast issued by an analyst and the actual earnings per share in quarter *t+1*, scaled by the share price at the end of quarter *t*. In specifications (4) to (6), the dependent variable is analyst forecast bias. Forecast error and bias are defined as in (Hong & Kacperczyk, 2010). Institutional investor distraction is defined over quarters from *t-3* to *t*, as in Kempf et al. (2016). Other distraction is defined as the value-weighted average of the distraction measure for all other firms in the analyst's portfolio in the same quarter, excluding the firm itself. Control variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are robust to clustering at the firm level.

	Forecast error$_{t+1}$			Forecast bias$_{t+1}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Distraction	1.901	1.893	1.834	1.705	1.678	1.357
	(4.51)	(4.64)	(4.24)	(4.22)	(4.31)	(3.31)
Other distraction	-0.359	-0.427	-0.291	-0.244	-0.302	-0.303
	(-4.84)	(-4.94)	(-3.01)	(-3.49)	(-3.74)	(-3.37)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry×quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	Yes	Yes	No
Brokerage FE	Yes	Yes	Yes	Yes	Yes	Yes
Analyst FE	No	Yes	No	No	Yes	No
Analyst × firm FE	No	No	Yes	No	No	Yes
N	908,748	907,842	871,860	908,748	907,842	871,860
R^2	0.42	0.43	0.49	0.28	0.29	0.37

TABLE A.1

Variable Definitions and Sources

This table defines the main variables used in the empirical analysis.

Variable name	Description	Source
Dependent variables		
Mean (median) forecast error_{t+1}	The absolute value of the difference between the consensus, i.e., the mean (median) forecast issued by analysts, and the actual earnings per share announced by the firm in quarter *t+1*, scaled by the share price at the end of quarter *t*, as in Hong & Kacperczyk (2010). I only retain forecasts issued or revised within 60 calendar days immediately preceding the earnings announcement date.	I/B/E/S
Mean (median) forecast bias_{t+1}	The difference between the consensus, i.e., the mean (median) forecast issued by analysts and the actual earnings per share announced by the firm in quarter *t+1*, scaled by the share price at the end of quarter *t*, as in Hong & Kacperczyk (2010). I only retain forecasts issued or revised within 60 calendar days immediately preceding the earnings announcement date.	I/B/E/S
Delay_{t+1}	The number of days between the end of quarter *t* and the first forecast issued by analysts for a firm in quarter *t+1*, averaged across across all analysts covering the firm in *t+1*, following Merkley et al. (2017).	I/B/E/S
Question length_{t+1}	The average length of questions asked by analysts in conference calls with managers in the three quarters after distraction, i.e., *t+1*, *t+2*, *t+3*. Length is measured as the ratio of words spoken by analysts to the number of questions asked during the call, following Merkley et al. (2017)	FactSet
Forecasts per analysts_{t+1}	The ratio of the total number of forecasts issued by analysts for a firm in quarter *t+1* to the number of analysts issuing at least one forecast in *t-1*, as defined by Merkley et al. (2017).	I/B/E/S
Δ Analysts_{t+1}	The difference between number of analysts entering and exiting in the quarter *t+1*, as per Merkley et al. (2017). Analyst exits are all analysts who issue a forecast in quarter *t*, but do not issue one for the next four quarters, or analysts who issue their last forecast for the firm in quarter *t*. Analyst entries are all analysts who issues a forecast in quarter *t+1* after not having issued one in the preceding four quarters, or analysts who issue their first forecast for the firm in quarter *t+1*	I/B/E/S

Continued

TABLE A.1

Continued

Variable name	Description	Source
Key independent variables		
Distraction	The average of institutional investor distraction for a firm over quarters t to t-3, as defined in Kempf et al. (2016).	
Other distraction	The value-weighted average of institutional investor distraction for all firms in analyst's portfolio other than the given firm over quarters t to t-3, as per (Kempf et al., 2016).	
Control variables: Firm-level		
Institutional ownership$_{t-1}$ (IO)	The fraction of the firm's stock owned by institutional investors as defined in Thomson Reuters 13f database at the end of quarter t-1.	Thomson Reuters 13f
Top 5 institutional owners' (IO) share$_{t-1}$	The fraction of the firm's stock owned by the five largest institutional investors as defined in the Thomson Reuters 13f database at the end of quarter t-1.	Thomson Reuters 13f
Log of market capitalization$_t$	The natural logarithm of the firm's stock price multiplied by the number of shares outstanding at the end of quarter t.	CRSP
Stock price return$_t$	The average return on the share of firm in quarter t.	CRSP
Return volatility$_t$	The standard deviation of the daily returns on the share of firm in quarter t.	CRSP
Log of book to market ratio$_t$	The natural logarithm of the firm's book value divided by its market capitalization (price times shares outstanding) at the end of quarter t.	CRSP, Compustat
Volatility of ROE$_t$	The variance of the residuals from an AR(1) model for a firm's return on equity using a 10-quarter series, following Hong & Kacperczyk (2010).	Compustat
Profit$_t$	The ratio of operating income at end of quarter t to the book value of the assets at the end of quarter t-1.	Compustat

Continued

TABLE A.1

Continued

Variable name	Description	Source
Control variables: Analyst-level		
High$_t$	Dummy variable equal to one if the firm is in the top quartile of market capitalization within the firms covered by the analyst in a given quarter t.	I/B/E/S, Compustat
Low$_t$	Dummy variable equal to one if the firm is in the bottom quartile of market capitalization within the firms covered by the analyst in the quarter t.	I/B/E/S, Compustat
General experience$_t$	Number of quarters for which the analyst has been active i.e. issued at least one forecast, till quarter t.	I/B/E/S
Firm-specific experience$_t$	Number of quarters for which the analyst has been active i.e. issued at least one forecast for the firm, till quarter t.	I/B/E/S
Forecast age$_t$	The number of days between the date on which forecast was issued and the end of the quarter t in which the forecast was issued.	I/B/E/S
Portfolio size$_t$	Number of firms for which the analyst issues at least one forecast in quarter t.	I/B/E/S
SIC2$_t$	Number of unique SIC2 industries of firms for which the analyst issues at least one forecast in quarter t.	I/B/E/S
Brokerage resources$_t$	Dummy variable equal to one if the analyst is working at a brokerage in top decile by number of analysts in the brokerage in quarter t.	I/B/E/S

Chapter 3

Gender Gap in Punishing Failure: Evidence from U.S. Patent Applications

3.1 Introduction

Do firms punish men and women for creative failures equally? The answer to this question has important implications for participation and performance of women in the labor market. If women are punished for failures resulting from experimentation at higher rates than equally performing men, they may be discouraged from entry into creative fields or might be incentivized to take lower risk. This, in turn, might lead to a gender gap in career outcomes even when the performance of both men and women is the same. Still, scant evidence exists on how firms respond to creative failures of their employees and how this response varies by gender.

One field where creative tasks are crucial to performance, and failure is common is innovation. From the perspective of inventors, experimentation is crucial to the process of innovation. Yet, outcomes of this process are uncertain and often, characterized by a high degree of failure (Holström, 1989). Firms, on the other hand, are incentivized to design contracts with high tolerance for failure in the short run, but which reward long-run success (Manso, 2011). Thus, while failure is unavoidable for an inventor, low tolerance thereof is sub-optimal for the firm (Tian & Wang, 2014).

To test for a gender gap in punishment in innovation, I examine an important and salient portion of the innovation process: a patent application. [1] Specifically, I analyze how failure to obtain a patent affects the careers of male and female inventors who are applying for patents for the first time. However, such an analysis is fraught with empirical challenges. First, it necessitates data which track the career outcomes of individuals over time and between firms. Second, it requires a measure of failure that can be used to compare different inventors employed at different firms. Third, failure in an application might be endogenously determined

[1] Patents add considerable market value to firms (Hall et al., 2005; Kogan et al., 2017). At the same time, an average patent application involves considerable uncertainty and a non-trivial probability of failure.

by unobserved characteristics, such as quality, of the underlying patent.

To address the first challenge, I compile a detailed dataset that links inventors who apply to the United States Patent and Trademark Office (USPTO) to their applications and also to the firms who employ them. My sample consists of 985,437 unique inventors who made their first applications between November 2001 and February 2013 and permits me to observe their movements across 34,698 firms, including 33,000 startups. I solve the second challenge by exploiting the systematic nature of the patenting process which implies that examiners are restricted to a certain set of decisions on applications which can then be compared across applicants. In particular, I focus on the inventors' first application to the Patent Office as the benchmark to compare different inventors. First-time patent applications have been shown to have important implications for the long-term growth of startups and are therefore particularly salient (Farre-Mensa et al., 2020). Finally, I use the differences in propensity between examiners to grant a patent or "leniency" to tackle the issue of endogenous failure. This approach relies on two features of the Patent Office: that the assignment of an application to an examiner is quasi-random; and that there is considerable variation in how examiners interpret set rules and thereby, decide to issue a patent or not (Lemley & Sampat, 2012; Farre-Mensa et al., 2020). Thus, implementing this approach permits me to causally identify how firms treat failure.

I document an economically sizable and persistent gender gap in punishment of failure. Specifically, firms are 9 p.p. more likely to fire female inventors for "as-good-as-random" patent rejections in comparison to their male counterparts. This corresponds to a 64% (=9/14%) higher probability of dismissal from employment in comparison to the average inventor. This effect is significant up to 5 years after the initial decision from the patent office. Female inventors who fail are less likely to be re-hired by other firms and are significantly more likely to exit innovation altogether. Upon examining the behavior of other employees within the firm, I find that women who face rejection in their first patent also obtain fewer coauthors. Overall, these results suggest that firms treat creative failure by men and women differently.

I further conduct two subsample analyses to rule out the possibility that reduction in inventor effort rather than punishment by the firm might drive these results. First, I compare startups where female-led teams fail with those where they succeed, and find that the former set of firms punish female prospective hires and even female employees who were *not* on the patenting team. This reflects a firm-level change in behavior towards female employees. Second, I find that independent female inventors neither face penalty in terms of labor market outcomes nor in collaborative activity with coauthors.

Finally, I attempt to quantify the cost of this gender gap in penalty and find that female inventors who suffer failure in initial patent applications lower both the quantity and quality of their innovative output. In particular, women who face rejections file 17% fewer applications and obtain 54% fewer patents compared to the unconditional mean.

Overall, my results show that even when faced with random failure, women get punished by their employers to a greater extent than men. Thereby, my results relate to several literatures. First, I contribute to the extensive literature which looks at gender gaps in labor market outcomes. [2] Traditionally, this literature has focused on higher rewards for men in terms of hiring, promotion and wages (Neumark et al., 1996; Goldin & Rouse, 2000; Carlsson, 2011; Goldin, 2014; Blau & Kahn, 2017). More recently, however, a small yet growing literature has studied how punishment also varies across men and women. Egan et al. (2017) document that women who engage in misconduct are much more likely to lose their jobs, but also less likely to be re-hired by other firms. Sarsons (2017) documents that female doctors face a much sharper drop in patient referrals relative to their male colleagues following a patient death. My paper contributes to this literature by studying how failures which are customary to the job result in much worse outcomes for women. Additionally, I am able to capture the effect of this punishment gap on further productivity.

I also contribute to the literature highlighting gender gap in participation in scientific fields and innovation. While prior studies have proposed biological (Hedges & Nowell, 1995), socio-cultural,(Hyde & Mertz, 2009) and environmental factors (Carrell et al., 2010; Miyake et al., 2010) as explanations for this gap, a big portion of this disparity still remains unexplained. I propose and provide evidence for a new explanation: that women are penalized much more than men for failures in producing creative output. My baseline results on dismissal from firms and exit from innovation directly connect to the under-participation of women in innovation.

Finally, I provide novel evidence that employers punish all members of a minority group for the failure of one individual in form of job dismissals as well as reduced hiring from that particular group. In doing so, this paper contributes to prior work which shows evidence for 'group' punishment for minorities in classroom (Okonofua & Eberhardt, 2015) as well as in updating beliefs about a group based on the outcome of one individual (Reuben et al., 2014; Sarsons, 2017).

The rest of this study is structured as follows. Section 3.2 discusses the institutional background and presents the sample construction. Section 3.3 outlines the empirical strategy implemented in this study. Section 3.4 documents the gender gap in punishment, considers alternative explanations, and quantifies the effect of this gap on productivity of inventors. Section 3.5 concludes.

3.2 Institutional Background and Data

3.2.1 Examination Process

The patent prosecution process begins when a new application is submitted by an inventor or a team of inventors to the USPTO. The Patent Office then sorts the application to the relevant "art unit", with each unit being a department of the Office specializing in a technological class. Both art units and examiners are highly specialized. Between November 2001 and February 2018, there were more than 13000 examiners employed at the Patent Office in approximately 1000 art units.

Once the application is forwarded to an art unit, the Supervisory Patent Examiner (SPE) then assigns the patent applications to examiners in the unit. The patent is typically assigned

[2] For literature surveys of this topic, please see Altonji & Blank (1999), Marianne (2011), Blau & Kahn (2017).

based on the number of cases pending with the examiner, the application number or on the filing date of the application (Shu et al., 2019). Furthermore, both interviews as well as prior studies indicate that within each art unit, the assignment to examiners is random and unrelated to applicant or firm characteristics (Lemley & Sampat, 2012; Farre-Mensa et al., 2020). While inventors and firms can tailor their applications such that it is likely to be sent to a given unit, they cannot affect the choice of the specific patent examiner who will review their application.

Each patent application consists of claims to novelty and the role of the examiner is to assess whether the claims should be allowed. To do so, the examiner conducts a thorough review of prior "art" or literature. After conducting the review, the examiner sends a "first-action letter", which is the first communication between the applicant and the examiner. Typically, a non-final rejection is issued with objections to the claims made by the applicant. The applicants can change their patent claims and respond to this initial decision.

Examiners, then, decide whether to approve these amended claims or to issue a final rejection. Applicants have an option to appeal a final rejection or file a continuation or divisional application. On average, the patent prosecution process takes about 3.4 years.

3.2.2 Data

I compile a comprehensive dataset which links firms to the individual inventors employed by them and to the applications filed by these inventors. The core of my data comprises of the Patent Examination (PatEx) dataset consisting of information on all patent applications filed with the USPTO between January 1981 and February 2018 (Graham et al., 2018b). This database contains uniquely identified examiners, their decisions on applications, the names of applicant inventors, and the name of the assignee, that is the individual or firm to whom the patent ownership is transferred. I only retain first-time utility applications filed between November 2001 and February 2013, as the coverage for data on rejected applications prior to this period is incomplete (Graham et al., 2018b; Tabakovic & Wollmann, 2018). I truncate the data at February 2013 so as to observe at least five years of effects of failure in patenting.

A central identifying assumption of this paper is the random assignment of applications within art units and so, I exclude cases where assignment is not random i.e. continuations, divisionals, and continuations-in-part. Additionally, applications where examiners do not issue a decision such as provisional, reissue, and patent co-operation treaty (PCT) applications are dropped.

In order to assemble the final dataset using PatEx, I need to overcome three central challenges: (1) uniquely identifying individual inventors, (2) identifying gender of these inventors, and (3) linking inventors to the firms where they are employed.

I begin by using an algorithm designed by Li et al. (2014) to distinguish between individuals with similar names. This algorithm uses a vector of different characteristics recursively to determine whether a given patent is likely to have been written by the inventor. This, in turn, allows me to uniquely and accurately identify inventors and their coauthors.

The next challenge is identifying the gender of the disambiguated inventors. I use state-level name frequency data from the Social Security Administration (SSA) to match inventors to their gender using their first name. A name is matched with a gender when the percentage of names in that state belonging to that gender is above 95%. Where the name cannot be matched to a gender using this dictionary or for inventors residing outside the United States, I use the cross-country dataset on frequency of names provided by the WIPO (World Intellectual Property Organization) (Lax Martínez et al., 2016). Overall, I match about 86% of all inventors (2,435,291 out of 2,814,389) from the original PatEx dataset.

The third challenge is matching inventors to firms, as these are not readily given in the PatEx dataset. I use the data provided by Arora et al. (2019) which link granted patents to the firms in the CRSP-Compustat. To link applications to firms, I use the Patent Assignee file in the PatEx database and use the code provided by the NBER patent data project which cleans and matches assignee names to firm names in the CRSP-Compustat dataset based on the degree to which the two names share unusual words.[3]

To identify startup firms, I implement the approach described by Farre-Mensa et al. (2020) and use firm names from Thomson One VentureXpert database. Next, I drop firms based outside the United States, not-for-profit entities like academic institutions or government agencies, and large publicly listed firms which are listed in the CRSP-Compustat dataset. I drop applicants which are not classified as "small business entity" in the PatEx dataset and thereby, construct the final sample.

Table 1 provides the summary statistics for my main sample. The final sample consists of 985,437 first-time patent applicant inventors with 34,698 unique firms of which 33,098 are startups. The average final rejection rate for a patent is 36% , while approximately, 16% of all first-time inventors are female.

3.3 Empirical Strategy

The key empirical challenge of this paper is disentangling the outcome i.e. "rejection" of the application, from the quality and other unobserved characteristics of the patent which might influence the examiner's final decision. I tackle this challenge twofold: first, I exploit the quasi-random assignment of patent examiners to applicants within each art unit, and second, I use the instrumental variables (IV) approach proposed by Farre-Mensa et al. (2020), wherein the instrument measures the "leniency" or the propensity of the examiner to grant a patent. Leniency is defined as the number of applications granted by a given examiner divided by the number of patents reviewed by her, prior to filing date of the focal patent. As this analysis focuses on outcomes after rejection, I subtract leniency from one constructing a variable I refer to as "strictness".

To better understand the intuition underlying this empirical approach, consider the following thought experiment. Suppose there are two nearly identical inventors A and B who apply to the Patent Office with a claim to a new technology: "automobile parts". The applications of these two inventors are then routed by the Office to the relevant art unit: "vehicles". Upon reaching the art unit, the two applications by A and B are randomly assigned to two distinct

[3] https://sites.google.com/site/patentdataproject/

examiners 1 and 2 respectively. 1 is a less "strict" examiner i.e. she is more likely to grant a patent on average, while 2 is less likely to approve the patent i.e. she is stricter. In such a scenario, the likelihood of A's patent application being approved increases, with the reverse holding true for B. In this example, being assigned to a lenient or a strict examiner effectively randomizes the success or failure of an application.

A 2SLS estimation with strictness as an instrumental variable approximates this thought experiment. Following Farre-Mensa et al. (2020), I estimate the first-stage of this 2SLS analysis in A.2. Here, I show that strictness is a credible instrument with a strong first stage, directly influencing the patent rejection rate. Additionally, as shown in A.3, being assigned to a stricter examiner is not affected by any inventor or examiner characteristic. I formalize my empirical approach in equation 3.1:

$$\begin{aligned} Inventoroutcomeijfat+k = \beta_1 E[Patentrejection|Firstactiondecision]_{ijfat} \\ +\beta_2 E[Patentrejection|Firstactiondecision]_{ijfat} \times Gender_i + \beta_3 Gender_i + \\ \phi X_{ijfat} + \nu_{a\tau} + \epsilon_{ijfat+k} \end{aligned} \tag{3.1}$$

where i refers to the first-time applicant inventor, j the firm which is employing the inventor, f examiners, a art units, and τ application year. t is the year of the first-action date. Following Farre-Mensa et al. (2020), I use the first-action date rather than the filing or grant date as the choice of a starting point to measure outcomes of rejection. This is mainly because the first-action letter contains detailed information indicating whether the application might be approved, thereby, affecting the behavior of the applicant.

The main coefficient of interest is β_2 which the measures the differences in outcomes between female and male first-time inventors who get rejected. A central identifying assumption underlying the analysis in equation 3.1 is that applicants and examiners are matched randomly. The only influence that the applicant has over the patent prosecution process is the technology of the patent and the time to file the patent. Therefore, I always include $\nu_{a\tau}$ - examiner art unit $\times$ application year fixed effects. X_{ijfat} is the vector of control variables. I provide a complete list of variable definitions in Appendix A.1.

3.4 Results

3.4.1 Main Results

Table 2 reports the baseline results. Panel A examines gender differences in the probability of being dismissed from employment when an inventor's first patent is rejected. Panel B studies whether an employee who is fired by her initial firm, then gains employment at another firm. Panel C considers whether a rejection is likely to affect exit from innovation. Employment (or dismissal) is defined as an indicator variable equal to one if the inventor is employed (or not) at the firm for at least 12 months, and zero otherwise. All first-stage regressions include art unit $\times$ application year fixed effects and the second-stage regressions include the number of claims as a control variable. Standard errors are clustered at the art unit level.

The results highlight asymmetries in the employment-related career outcomes of male and female inventors. While firms do not dismiss inventors immediately after failure, the effect is both significant and economically sizable upon considering longer time horizons. Specifically, female inventors who receive rejection are are 9 p.p. more likely to be fired within three years after the first-action date. This effect is economically large at 64% (=9/14%) of the unconditional mean rate of dismissal. This gap in dismissal is persistent, increasing further to 9.1 p.p. upon considering the first five years.

The estimates in Panel B uncover a similar disparity in being re-hired by the same or another firm, conditional upon being dismissed by the initial firm. Taken together, Panels A and B reflect large and persistent gaps not only within the outcomes at the first place of employment but also for further labor market outcomes.

Panel C indicates that these effects extend to exit from innovation, wherein rejected female inventors are 9.3 p.p. more likely to stop producing innovative output in comparison to their male counterparts. In other words, a female inventor is about 155% (=9.3/6%) more likely to exit innovation when her application is rejected as a result of being randomly assigned to a stricter examiner, compared to the average inventor.

In addition to dismissing or "firing" employees, firms might potentially withhold resources or promotions from underperforming employees. While I do not directly observe the resource allocation within the firm, I study an important yet related outcome variable in Table 3: co-authorship with other inventors in the firm. Innovation is rarely produced by individual inventors, with teams being a first-order human capital resource in firms. Hence, co-authors provide an important proxy for the resources available to the inventor in producing further innovation. (Baghai et al., 2018).

Panel A indicates that rejected female inventors also produce innovation with fewer coauthors as compared to their male counterparts. More strikingly, this effect is observed in data immediately with number of co-authors per patent dropping by 0.122 for female inventors. While this might seem like a modest effect, the mean number of inventors filing an application in my sample including the focal inventor herself is 1.5, so the drop in coauthors corresponds to a decrease in resources of 25% = ($0.122/0.5 \times 100$). The magnitude of this effect further increases to 0.18 or 34% of the unconditional mean of coauthors over 5 years after rejection.

In Panel B, I analyze whether this effect is driven exclusively by a decline in male co-authors. Though the decline in female co-authors is much smaller at 3% = ($0.005/0.14 \times 100$), it is nonetheless significant. Importantly, upon failing in their initial patent application, female inventors see a reduction in collaborations with both male and female colleagues.

In Panel C, I find that female patent applicants are less likely to receive lead position on patent applications after failure. While the ordering of names on a patent is not necessarily linked to greater monetary compensation, it is the result of discussion between co-authors, with the author with the most contribution typically receiving the lead position (Seymore, 2006). Thereby, the estimates from Panel C are informative from perspective of relative position of the inventors in her team.

Overall, the results presented so far strongly support the interpretation that male and female inventors experience different degrees of punishment when faced with rejection on their initial applications.

3.4.2 Effect of Rejection on Treatment of Other Female Inventors

My results on employment and co-authorship outcomes show that female inventors face a larger penalty compared to their male counterparts, even when rejection is "as-good-as-random" i.e. exogenous to the underlying quality of the application as well as the applicant. Yet, these results cannot fully indicate whether the documented outcomes are caused by discriminatory 'punishment' for failure by the firm or reduced effort by the inventor in face of rejection.

In order to better interpret the results presented so far, I construct a direct test of differential treatment by the firm using a subsample of startups whose first patent applications had a lead female inventor. Specifically, I compare the startups whose female-led first applications were rejected with those where the initial applications succeeded, and analyze their subsequent decisions vis-à-vis the female inventors who were *not* on the initial applicant team. This test is based on two sets of evidence from prior literature. First, individuals use salient success or failure of one member of a social group to update their beliefs about *all* members of that group (Reuben et al., 2014; Okonofua & Eberhardt, 2015; Sarsons, 2017). Second, startups' first patent applications are especially important to their long-term success in raising venture capital or going public via an initial public offering (IPO) and therefore, serves as a salient reference point that the firm might use to update its beliefs (Farre-Mensa et al., 2020).

I thereby estimate a variation of equation (3.1) aggregated at the startup level:

$$Decision_{fjat+k} = \beta_1 E[Patentrejection|Firstactiondecision]_{fjat} + \phi X_{fjat} + \nu_{a\tau} + \epsilon_{fjat+k} \quad (3.2)$$

where the main outcome variable is the decision by the startup f in year $t+k$, k years after the date of first action by examiner j in art unit a. All other variables are as defined in equation 3.1.

As the first outcome variable, I look at the percentage of female inventors hired out of all inventors hired by the firm. In other words, I examine the choice of successful and unsuccessful firms within this subsample to hire female vs. male inventors after their initial application. Next, I follow the same intuition and consider the percentage of female inventors fired out of all those who are fired. It is relevant to note that the estimation in Panel B exclude those inventors who are on the team filing the initial patent application. Finally, I examine the percentage of applications with female employees listed as primary or lead inventors.

Table 4 reports the results from estimating equation 3.2. All three outcome variables indicate that in startups where female-led teams fail, the outcomes for other female employees also worsen. *Ceteris paribus*, firms with failed applications hire fewer female inventors compared to male inventors, fire more female employees, and choose fewer female inventors as lead inventors on their applications. In sum, the evidence from Table 4 lends more credence to an explanation based on firm changing its treatment rather than inventors adjusting their effort, as inventors not directly linked to the initial rejection face more negative effects as well.

3.4.3 Independent Inventors

So far, the evidence suggests that female inventors are punished by their employers for "as-good-as-random" failures to a greater extent than men. However, to further rule out the explanation that these results cannot be explained by reduced effort by female inventors upon receiving a rejection, I conduct an additional subsample analysis on inventors not affiliated with any firm i.e. "independent inventors". The conjecture underlying this analysis is that if the observed gender gap in outcomes is driven by inventor-level factors such as gender differences in risk preferences, this reduction must also be reflected in the subsample of independent inventors.

I begin by constructing a subsample of inventors defined as independent if their initial application is not assigned to any firm. After identifying approximately 150,000 such inventors, I filter the sample to focus on rejections (Farre-Mensa et al., 2020) and removing any observations containing missing variables for the main dependent variables. My final sample, thus, consists of 51,612 unique observations. For this analysis, I focus on four main variables measured over the first five years from the first-action date: exit from innovation, probability of being hired, number of co-authors, and the probability of being a lead inventor on an application.

Table 5 presents the results. As hypothesized, I find that independent female inventors are not any likelier to exit innovation compared to male inventors. Similarly, while women are unconditionally less likely to be hired by firms, there is no gender difference in the probability of entering employment with a firm conditioning upon rejection. Finally, independent female inventors neither experience a significant drop in the number of co-authors, nor are less likely to be lead inventors on future patent applications.

While the two subsample analyses provide considerable evidence that differences in how firms treat male and female employees is likelier to explain the results, it is important to introduce the caveat that selection into neither samples is exogenous. Nonetheless, the above results strongly suggest that decisions by the firm play a key role in introducing the gender gap in outcomes after initial failure.

3.4.4 Firm and Inventor-level Characteristics

To further lend credence to the argument that the worse career outcomes faced by women are driven by firm-level factors as opposed to inventor-level decisions, I conduct two additional tests which exploit the heterogeneity in firm and inventor characteristics. Specifically, I use two measures of conservatism, one which varies at the firm-level and other which mainly varies at the inventor-level.

First, I consider whether adverse career outcomes after failure are observed for inventors working in firms with a more gender egalitarian culture. Specifically, I use the measure of a firm's "sexist culture" as defined by (Lins et al., 2020), with the presence of a female executive among the company's most highly-paid executives being a proxy for firm sexism. I term this measure *Egalitarian*, with the variable being equal to one if at least one of the top five highest paid executives is a women, and zero otherwise. The data on the board composition of firms, gender of the executives therein, and their compensation is obtained from BoardEx and Execucomp databases and I am able to construct this measure for 1,315 publicly listed firms in my sample. As shown by (Lins et al., 2020), firms which score high on this measure earned positive excess returns during the #MeToo movement and in the aftermath of revelation of the Harvey Weinstein scandal.

Thereafter, I consider an inventor-level factor which might affect the response of female inventors to failure, namely, whether the inventor resides in the Southern United States. Individuals in the Southern US have been shown to have more traditional views of gender norms (D'Acunto, 2019). Given that social norms play a key role in whether women persist after initial failure (Gneezy et al., 2009), I conjecture that female inventors from these states might be more likely to voluntary leave employment and exit innovation. Thus, this measure would allow me to study the role of female risk-aversion in adverse career outcomes observed.

Panels A and B of Table 6 report the results. In this Table, I re-estimate the baseline specification in (3.1) with measures of conservatism as an additional interaction terms. I find that female inventors in more egalitarian firms are significantly less likely to experience job separation as compared to those in less egalitarian firms in three and five years after initial patent rejection. By contrast, female inventors residing in Southern US, as compared to elsewhere in the US, are not less likely to experience dismissals. This result is striking given that female inventors in the South are unconditionally more likely to experience job dismissals. These results further support my interpretation that the differential dismissal from employment is driven by firm-level factors as opposed to individual-level factors.

3.4.5 Effect on Innovative Output

Finally, I attempt to quantify the decline in productivity of female inventors who receive rejections on their initial patent applications. Table 7 reports the estimates of equation 3.1 with measures of quantity and quality of the innovative output as the dependent variables. In all columns, the dependent variables are the average values per year over the first five years after the first-action date.

The results in Table 7 highlight the decline in patenting activity by female inventors. I find that there is an economically sizable decline in the quantity of innovation with the number of applications and patents filed by the inventor declining by 17% (=4/23 $\times$ 100) and 54% (=7/13 $\times$ 100) relative to the unconditional means. In the next four columns, I consider measures of patent quality. In column (3), I document a large and persistent decline in the number of forward citations received by the inventors' patents. The measures in columns (4), (5), and (6) are strongly correlated with the value of the patent and are aimed at measuring breakthrough technologically novel innovations. Here, I find a similar reduction in the generality, originality, and number of highly cited or "top ten" patents, produced by the inventor.

Overall, these estimates point to a sizable decline in female inventors' innovative output resulting from assignment to a stricter examiner on the first patent.

3.5 Conclusion

I document gender differences in punishment of creative failures. Using a detailed dataset that covers the universe of first-time inventors applying to the United States Patent and Trademark Office, I show that firms are more likely to fire female inventors for near-random rejections. My empirical approach uses the random assignment of patents to examiners, thus ensuring that differences in quality of patents cannot explain the results. I additionally find that employees of the firm also punish female inventors by reducing co-authorships. Using two subsample analyses, I provide suggestive evidence that the gap stems from firm actions than from decreased inventor effort. Furthermore, I find that women who fail in their first-time applications reduce their overall innovative output and are more likely to exit innovation.

The pervasiveness of creative tasks in workplaces implies that reduced tolerance for female creative failures might limit the participation of women in creative and scientific fields including academia. This gender gap in punishment might also have ramifications for the scope of activities that women undertake when they participate in these fields. This avenue is particularly promising one for further research in my view.

TABLE 1
Summary Statistics

This table presents summary statistics for the main variables. The sample consists of all inventors who applied for a patent for the first time between 1st November 2001 and 28th February 2013. All variables are defined in Appendix A.1.

	N	Mean	Std. Dev.
Patent rejected	985,437	0.36	0.48
Female	985,437	0.16	0.42
Strictness	985,437	0.33	0.21
Patent Scope	627,901	3.30	3.0
Career Outcomes			
Dismissed from employment			
1 year	985,437	0.05	0.15
3 years	877,061	0.15	0.34
5 years	824,437	0.29	0.60
Dismissed and re-hired			
1 year	985,437	0.11	0.30
3 years	877,061	0.31	0.59
5 years	824,437	0.35	0.78
Exit from innovation			
1 year	985,437	0.04	0.04
3 years	877,061	0.06	0.13
5 years	824,437	0.12	0.20
Co-authorship			
Co-authorship with inventors in firm			
1 year	985,437	0.52	1.49
3 years	877,061	0.78	1.65
5 years	824,437	0.53	1.83

Continued

TABLE 1

Summary Statistics

Continued

	N	Mean	Std. Dev.
Co-authorship with female inventors in firm			
1 year	985,437	0.14	0.21
3 years	877,061	0.14	0.30
5 years	824,437	0.09	0.20
Lead inventor on patent			
1 year	985,437	0.64	0.14
3 years	877,061	0.48	0.09
5 years	824,437	0.77	0.40
Innovative Output			
No. of applications	824,437	0.23	1.18
No. of patents	824,437	0.13	0.82
Forward citations	824,437	0.02	0.27
Generality	533,262	0.030	0.18
Originality	533,262	0.028	0.19
Top Ten	533,262	0.00	0.04

TABLE 2
Gender Differences in Career Outcomes after Failure

This table reports the results of estimating equation (3.1) as a linear probability model with job dismissal, dismissal and re-hiring, and exit from innovation as the dependent variables respectively. All variables are defined in Appendix A.1. t-statistics, reported in parentheses, are based on standard errors that are clustered at the art unit level.

	1 year	3 years	5 years
Panel A: Dismissed from employment			
Patent rejected × female	0.009	0.090	0.091
	(1.15)	(3.09)	(3.11)
Patent rejected	0.029	0.035	0.043
	(0.56)	(1.40)	(1.50)
Female	0.008	0.032	0.051
	(2.01)	(2.15)	(2.03)
N	353,457	327,654	291,612
R^2	0.78	0.69	0.71
Panel B: Dismissed and re-hired			
Patent rejected × female	-0.013	-0.009	-0.001
	(-0.90)	(-1.94)	(-2.14)
Patent rejected	-0.001	0.000	0.000
	(0.31)	(0.89)	(0.89)
Female	-0.029	-0.034	-0.042
	(-2.20)	(-9.09)	(-9.49)
N	353,457	327,654	291,612
R^2	0.55	0.24	0.41
Panel C: Exit from innovation			
Patent rejected × female	0.005	0.093	0.093
	(1.58)	(2.42)	(2.44)
Patent rejected	0.034	0.038	0.039
	(1.00)	(1.40)	(1.44)
Female	0.106	0.107	0.111
	(7.21)	(7.33)	(6.52)
N	353,457	327,654	291,612
R^2	0.68	0.55	0.61

TABLE 3

Gender Differences in Co-Authorships after Rejection

This table reports the results of estimating equation (3.1) with co-authorship with other inventors in the firm, co-authorship with female inventors in the firm, and being a lead inventor as the dependent variables respectively. All variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are based on standard errors that are clustered at the art unit level.

	1 year	3 years	5 years
Panel A: Co-authorship with inventors in firm			
Patent rejected × female	-0.122	-0.124	-0.180
	(-1.68)	(-1.98)	(-1.85)
Patent rejected	0.204	0.348	0.219
	(0.48)	(0.24)	(0.51)
Female	-0.109	-0.142	-0.403
	(-1.99)	(-3.12)	(-2.04)
N	328,715	292,556	275,003
R^2	0.29	0.45	0.48
Panel B: Co-authorship with female inventors in firm			
Patent rejected × female	-0.004	-0.005	-0.005
	(-1.83)	(-1.66)	(-1.90)
Patent rejected	0.025	0.031	0.024
	(1.03)	(1.11)	(1.05)
Female	0.021	0.030	0.030
	(0.44)	(0.52)	(0.43)
N	328,715	292,556	275,003
R^2	0.05	0.08	0.05
Panel C: Lead inventor on patent			
Patent rejected × female	-0.112	-0.140	-0.140
	(-3.40)	(-4.12)	(-4.13)
Patent rejected	-0.005	-0.008	-0.009
	(-1.39)	(-1.52)	(-1.52)
Female	-0.150	-0.166	-0.166
	(-5.59)	(-5.98)	(-6.01)
N	328,715	292,556	275,003
R^2	0.34	0.41	0.41

TABLE 4

Effect of Failure on Treatment of Other Female Inventors

This table reports the results of estimating equation (3.2) to examine the effect of a failure of the first patent application by a startup on its subsequent decisions with respect to hiring and dismissing male and female inventors, and assigning male and female inventors to lead its applications. The equation is estimated on a subsample of startups whose first patent application has a female lead inventor. All variables are defined in Appendix A.1. t-statistics, reported in parentheses, are based on standard errors that are clustered at the art unit level.

	1 year	3 years	5 years
Panel A: Subsequent hires			
Patent rejected	-0.007	-0.004	-0.009
	(-2.78)	(-2.98)	(-3.10)
Controls	Yes	Yes	Yes
N	3,440	3,440	3,440
R^2	0.68	0.70	0.72
Panel B: Subsequent dismissals			
Patent rejected	0.029	0.010	0.003
	(1.89)	(1.03)	(1.38)
Controls	Yes	Yes	Yes
N	3,440	3,440	3,440
R^2	0.55	0.59	0.60
Panel C: Female lead inventors			
Patent rejected	-0.029	-0.050	-0.034
	(-1.87)	(-1.90)	(-1.35)
Controls	Yes	Yes	Yes
N	3,440	3,440	3,440
R^2	0.35	0.35	0.48

TABLE 5
Gender Differences in Effect of Failure on Independent Inventors

This table estimates equation (3.1) with exit from innovation, probability of being hired, number of co-authors, and the probability of being a lead inventor as dependent variables for a sub-sample of independent inventors. Dependent variables are measured with over the first five years from the first-office action date. All variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are based on standard errors that are clustered at the art unit level.

	Exit from innovation	**Hired**	**Co-authorship**	**Lead inventor**
	(1)	(2)	(3)	(4)
Patent rejected × female	0.010	0.003	-0.091	-0.090
	(1.57)	(0.13)	(-1.22)	(-1.63)
Patent rejected	0.086	-0.008	0.047	0.001
	(2.49)	(-1.92)	(0.95)	(0.04)
Female	0.210	-0.002	-0.186	-0.003
	(10.39)	(2.88)	(-3.10)	(-1.75)
N	51,612	51,612	51,612	51,612
R^2	0.69	0.44	0.38	0.35

TABLE 6

Interaction with Measures of Conservatism

This table reports the results of estimating equation (3.1) as a linear probability model with measures of firm-level and individual-level conservatism as interaction terms, and job dismissal as the main dependent variable. All variables are defined in Appendix A.1. t-statistics, reported in parentheses, are based on standard errors that are clustered at the art unit level.

	Dismissed from employment		
	1 year	3 years	5 years
Panel A: Egalitarian			
Patent rejected × female × egalitarian	-0.092	-0.238	-0.505
	(-0.70)	(-2.24)	(-2.11)
Female	-0.003	0.004	-0.208
	(-0.20)	(0.24)	(-0.08)
Patent rejected	0.003	0.000	0.000
	(0.63)	(0.32)	(0.82)
Female	0.014	0.015	0.023
	(2.06)	(2.55)	(2.78)
Female × egalitarian	0.080	0.004	0.075
	(0.68)	(0.37)	(1.62)
Patent rejected × egalitarian	0.006	0.067	0.142
	(0.28)	(0.47)	(0.51)
N	193,502	152,441	110,305
R^2	0.39	0.43	0.57
Panel B: Southern inventor			
Patent rejected × female × southern inventor	-0.039	-0.075	-0.085
	(-0.71)	(-1.03)	(-0.27)
Southern inventor	0.059	0.067	0.085
	(0.68)	(0.37)	(0.60)
Patent rejected	0.010	0.003	0.011
	(1.50)	(0.15)	(0.32)
Female	0.003	0.062	0.157
	(2.16)	(2.48)	(4.04)
Female × southern inventor	0.087	0.093	0.106
	(0.67)	(2.25)	(2.46)
Patent rejected × southern inventor	0.005	0.009	0.002
	(0.40)	(1.93)	(2.14)
N	353,457	327,654	291,612
R^2	0.32	0.52	0.77

TABLE 7
Effect of Failure on Innovative Output by Female Inventors

This table estimates equation 3.1 with measures of innovative output i.e. number of applications, number of patents, forward citations, generality, originality, and top ten cited patents as dependent variables. Dependent variables are the average values per year over the first five years after the first-action date. All variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are based on standard errors that are clustered at the art unit level.

	No. Applications	**No. Patents**	**Forward Citations**	**Generality**	**Originality**	**Top Ten**
	(1)	(2)	(3)	(4)	(5)	(6)
Patent rejected × female	-0.040	-0.070	-0.148	-0.093	-0.139	-0.109
	(-9.21)	(-3.09)	(-1.93)	(-2.09)	(-1.66)	(-8.90)
Patent rejected	-0.110	-0.425	-0.202	-0.563	-0.087	-0.115
	(-11.53)	(-5.10)	(-3.34)	(-1.89)	(-4.21)	(-2.48)
Female	-0.089	-0.310	-0.086	-0.160	-0.102	-0.099
	(-10.66)	(-2.46)	(-1.45)	(-3.03)	(-4.98)	(-7.39)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	275,003	275,003	275,003	275,003	275,003	275,003
R^2	0.54	0.49	0.13	0.30	0.39	0.15

Appendix 3.A Variable Descriptions

TABLE A.1

Variable Definitions and Sources

This table defines the main variables used in the empirical analysis.

Variable name	Description	Source
Dependent variables		
Patent rejected	An indicator variable equal to one if an application did not result in a successful patent grant and zero otherwise.	PatEx
Strictness	Number of patent applications rejected by the examiner divided by the number of applications reviewed, prior to the focal application	PatEx
Female	An indicator variable equal to one if the inventor is classified by the matching algorithm as a female and zero otherwise. See Section 3.2.2 for details of the process for matching inventors to their genders using their names.	PatEx, WIPO, Social Security Administration
Patent Scope	Number of independent claims allowed in the patent.	Patents Claims Dataset
Career Outcomes		
Dismissed from employment	An indicator variable equal to one if the inventor does not submit an application with the firm as an assignee for at least 12 consecutive months between year of first action t and year $t+k$, where $k = 1, 3, 5$ years, and zero otherwise.	PatEx, Arora et al. (2019)
Dismissed and re-hired	An indicator variable equal to one if the inventor is dismissed from her initial firm and then, submits more than 2 applications with another firm between year of first action t and year $t+k$, where $k = 1, 3, 5$ years, and zero otherwise.	PatEx, Arora et al. (2019)
Exit from innovation	An indicator variable equal to one if the inventor does not submit an application for at least 24 consecutive months between year of first action t and year $t+k$, where $k = 1, 3, 5$ years, and zero otherwise.	PatEx

Continued

TABLE A.1

Variable Definitions and Sources

This table defines the main variables used in the empirical analysis.

Variable name	Description	Source
Co-authorship		
Co-authorship with inventors in firm	Number of co-authors who are employed at the same firm f as the inventor i, divided by the number of applications filed by inventor i between year of first action t and year $t+k$, where $k = 1, 3, 5$ years. Employment at a firm is defined as an indicator variable equal to one if the inventor does not submit an application with the firm as an assignee for at least 12 consecutive months between year of first action t and year $t+k$, where $k = 1, 3, 5$ years, and zero otherwise.	PatEx, Arora et al. (2019)
Lead inventor on patent	Number of applications on which the inventor is the "lead" inventor or the first inventor, divided by the total number of applications filed by the inventor i between year of first action t and year $t+k$, where $k = 1, 3, 5$ years.	PatEx
Co-authorship with female inventors in firm	Number of female co-authors who are employed at the same firm f as the inventor i, divided by the number of applications filed by inventor i between year of first action t and year $t+k$, where $k = 1, 3, 5$ years. Employment at a firm is defined as an indicator variable equal to one if the inventor does not submit an application with the firm as an assignee for at least 12 consecutive months between year of first action t and year $t+k$, where $k = 1, 3, 5$ years, and zero otherwise.	PatEx, Arora et al. (2019)
Measures of Conservatism		
Egalitarian	An indicator variable equal to one if at least one of the top five highest-paid executives at the firm is a woman, and zero otherwise.	Lins et al. (2020), BoardEx, Execucomp
Southern inventor	An indicator variable equal to one if the inventor resides in a state which is a part of region Southern United States as defined by the United States Census Bureau	PatEx, U.S. Census Bureau

Continued

TABLE A.1

Variable Definitions and Sources

This table defines the main variables used in the empirical analysis.

Variable name	Description	Source
Innovative Output		
No. of applications	Number of patent applications filed by inventor i between year of first action t and year $t+k$, where $k = 1, 3, 5$ years.	PatEx
No. of patents	Number of patents granted to inventor i between year of first action t and year $t+k$, where $k = 1, 3, 5$ years.	PatEx
Forward citations	Number of citations received by the inventor i's patents in the five years after grant date divided by the number of patents granted to the inventor between year of first action t and year $t+k$, where $k = 1, 3, 5$ years.	PatentsView
Generality	One minus the Herfindahl-Hirschman Index (HHI) of the forward citations to the given patent, where the HHI is calculated over the four-digit International Patent Classification (IPC) classes, divided by number of patents issued to the inventor i between year of first action t and year $t+k$, where $k = 1, 3, 5$ years. Generality is normalized by the average generality score for all the other patents granted in the same year and belonging to the same 3-digit technology class.	PatentsView
Originality	One minus the Herfindahl-Hirschman Index (HHI) of the backward citations to the given patent, where the HHI is calculated over the four-digit International Patent Classification (IPC) classes, divided by number of patents issued to the inventor i between year of first action t and year $t+k$, where $k = 1, 3, 5$ years. Originality is normalized by the average originality score for all the other patents granted in the same year and belonging to the same 3-digit USPTO technology class.	PatentsView

Continued

TABLE A.1

Variable Definitions and Sources

This table defines the main variables used in the empirical analysis.

Variable name	Description	Source
Top Ten	Number of a worker's patents that end up in the top 10% among all patents from the same year and 3-digit USPTO technology class.	PatentsView
Subsample: Start-up firms with female lead inventors on first application		
Subsequent hires	Number of female inventors hired by the firm between years t and $t+5$ divided by the number of total inventors hired by the firm during the same time period. An inventor i is classified as "hired" in year t if she did not assign a patent to any firm previously and then, assigned more than 2 patent applications to the firm f in year t.	PatEx, Arora et al. (2019)
Subsequent dismissals	Number of female inventors dismissed from the firm between years t and $t+5$ divided by the number of total inventors hired by the firm during the same time period. Dismissal is an indicator variable equal to one if the inventor does not submit an application with the firm as an assignee for at least 12 consecutive months.	PatEx, Arora et al. (2019)
Female lead inventors	Number of female inventors who are lead inventors on the firm f's applications divided by the number of total applications filed by the firm between years t and $t+5$. A "lead" inventor is an inventor whose name appears first on the application.	PatEx, Arora et al. (2019)
Subsample: Independent inventors		
Hired	An inventor i is classified as "hired" in year t if she did not assign a patent to any firm previously and then, assigned more than 2 patent applications to the firm f between years t and $t+5$.	PatEx, Arora et al. (2019)

TABLE A.2

Instrumental Variable: First-Stage Results

This table reports the results of estimating the first-stage equation of the 2SLS analysis, wherein the first stage regresses the final abandonment or rejection of the given patent on the prior rejection rate of the patent examiner. Claims is equal to the natural logarithm of the number of independent claims in the patent at the time of initial filing. Examiner experience is the natural logarithm of number of applications reviewed by the examiner prior to the focal application. Examiner grades are the official positions or "grades" of the examiners within the USPTO, with grades ranging from 9 (lowest) to 15 (highest). All other variables are defined in Appendix A.1. t-statistics, reported in parentheses, are based on standard errors that are clustered at the art unit level.

	First patent application rejected?				
	(1)	(2)	(3)	(4)	(5)
Rejection rate	0.052	0.051	0.055	0.039	0.039
	(37.50)	(37.69)	(27.43)	(19.32)	(21.50)
Inventor characteristics					
Gender		0.022			
		(12.83)			
Team experience			-0.056		
			(-28.15)		
Claims				-0.045	
				(-14.55)	
Examiner characteristics					
Examiner experience					0.022
					(1.69)
Examiner grade GS-9					0.005
					(0.01)
Examiner grade GS-11					0.001
					(0.04)
Examiner grade GS-12					0.001
					(0.16)
Examiner grade GS-13					0.003
					(0.05)
Examiner grade GS-14					0.000
					(0.000)
Examiner grade GS-15					0.000
					(0.000)
Art unit $\times$ year FE	Yes	Yes	Yes	Yes	Yes
Technology $\times$ year FE	No	No	No	Yes	Yes
F-test: IV = 0	1034.19	1148.91	782.14	550.14	421.30
N	984,650	983,333	984,650	996,985	996,986
R^2	0.16	0.16	0.17	0.24	0.23

TABLE A.3
Instrumental Variable: Validity

The table reports the results of regressing the rejection rate of the examiner on the characteristics of the inventor, the application, or the examiner. Claims is equal to the natural logarithm of the number of independent claims in the patent at the time of initial filing. Examiner experience is the natural logarithm of number of applications reviewed by the examiner prior to the focal application. Examiner grades are the official positions or "grades" of the examiners within the USPTO, with grades ranging from 9 (lowest) to 15 (highest). All other variables are defined in Appendix A.1. *t*-statistics, reported in parentheses, are based on standard errors that are clustered at the art unit level.

	Instrumental variable: Examiner strictness			
	(1)	(2)	(3)	(4)
Inventor characteristics				
Gender	-0.002			
	(-0.98)			
Team experience		0.077		
		(1.02)		
Claims			-0.000	
			(-0.64)	
Examiner characteristics				
Examiner experience				-0.019
				(-1.88)
Examiner grade GS-9				-0.001
				(-2.42)
Examiner grade GS-11				-0.004
				(-1.09)
Examiner grade GS-12				-0.005
				(-4.13)
Examiner grade GS-13				-0.000
				(-1.60)
Examiner grade GS-14				-0.011
				(-1.92)
Examiner grade GS-15				-0.013
				(-1.81)
Art unit × year FE	Yes	Yes	Yes	Yes
Technology × year FE	No	No	No	Yes
N	983,333	984,650	984,650	996,986
R^2	0.53	0.70	0.66	0.89

Bibliography

Abramova, I., Core, J., & Sutherland, A. (2017). Institutional investor attention and demand for inconsequential disclosures. *Working Paper*.

Altonji, J. G. & Blank, R. M. (1999). Race and gender in the labor market. *Handbook of labor economics*, 3, 3143–3259.

Arora, A., Belenzon, S., & Sheer, L. (2019). Back to basics: Why do firms invest in scientific research?

Baghai, R. P., Silva, R. C., & Ye, L. (2018). Bankruptcy, team-specific human capital, and productivity: Evidence from us inventors. *Stockholm School of Economics Working Paper*.

Baker, M. & Wurgler, J. (2013). Chapter 5 - Behavioral corporate finance: An updated survey. volume 2 of *Handbook of the Economics of Finance* (pp. 357 – 424). Elsevier.

Balakrishnan, K., Billings, M. B., Kelly, B., & Ljungqvist, A. (2014). Shaping liquidity: On the causal effects of voluntary disclosure. *Journal of Finance*, 69(5), 2237–2278.

Basu, R. S., Pierce, S., & Stephan, A. (2017). The effect of investor inattention on voluntary disclosure. *Working Paper*.

Becker, G. S. (1957). *Economics of Discrimination*. Chicago, IL: University of Chicago Press.

Bell, A., Chetty, R., Jaravel, X., Petkova, N., & Van Reenen, J. (2018). Who becomes an inventor in America? The importance of exposure to innovation. *The Quarterly Journal of Economics*, 134(2), 647–713.

Berle, A. A. & Means, G. C. (1932). *The Modern Corporation and Public Property*. New York: Harcourt, Brace & World.

Blau, F. D. & Kahn, L. M. (2017). The gender wage gap: Extent, trends, and explanations. *Journal of Economic Literature*, 55(3), 789–865.

Bordalo, P., Gennaioli, N., & Shleifer, A. (2015). Salience theory of judicial decisions. *The Journal of Legal Studies*, 44(S1), S7–S33.

Budish, E., Roin, B. N., & Williams, H. (2015). Do firms underinvest in long-term research? Evidence from cancer clinical trials. *American Economic Review*, 105(7), 2044–85.

Carlsson, M. (2011). Does hiring discrimination cause gender segregation in the swedish labor market? *Feminist Economics*, 17(3), 71–102.

Carrell, S. E., Page, M. E., & West, J. E. (2010). Sex and science: How professor gender perpetuates the gender gap. *The Quarterly Journal of Economics*, 125(3), 1101–1144.

Cen, L., Ornthanalai, C., & Schiller, C. M. (2017). Navigating Wall Street: Career concerns and analyst transitions from sell-side to buy-side. *Working Paper*.

Comenetz, J. (2016). Frequently occurring surnames in the 2010 Census. *United States Census Bureau*.

Cook, L. D. & Kongcharoen, C. (2010). *The Idea Gap in Pink and Black*. Working Paper 16331, National Bureau of Economic Research.

Cooper, R. A., Day, T. E., & Lewis, C. M. (2001). Following the leader: A study of individual analysts' earnings forecasts. *Journal of Financial Economics*, 61(3), 383–416.

D'Acunto, F. (2019). Identity and choice under risk. *Available at SSRN 3466626*.

Darling-Hammond, S., Michaels, E. K., Allen, A. M., Chae, D. H., Thomas, M. D., Nguyen, T. T., Mujahid, M. M., & Johnson, R. C. (2020). After the china virus went viral: Racially charged coronavirus coverage and trends in bias against asian americans. *Health Education & Behavior*.

Derrien, F. & Kecskés, A. (2013). The real effects of financial shocks: Evidence from exogenous changes in analyst coverage. *Journal of Finance*, 68(4), 1407–1440.

Diether, K. B., Malloy, C. J., & Scherbina, A. (2002). Differences of opinion and the cross section of stock returns. *Journal of Finance*, 57(5), 2113–2141.

Egan, M. L., Matvos, G., & Seru, A. (2017). *When Harry fired Sally: The double standard in punishing misconduct*. Technical report, National Bureau of Economic Research.

Fama, E. F. & French, K. R. (2008). Dissecting anomalies. *Journal of Finance*, 63(4), 1653–1678.

Farre-Mensa, J., Hegde, D., & Ljungqvist, A. (2020). What is a patent worth? evidence from the us patent "lottery". *Journal of Finance*, 75(2), 639–682.

Fleischer, V. (2010). Regulatory arbitrage. *Texas Law Review*, 89, 227.

Galasso, A. & Schankerman, M. (2014). Patents and cumulative innovation: Causal evidence from the courts. *The Quarterly Journal of Economics*, 130(1), 317–369.

Gans, J. S., Hsu, D. H., & Stern, S. (2008). The impact of uncertain intellectual property rights on the market for ideas: Evidence from patent grant delays. *Management Science*, 54(5), 982–997.

Garel, A., Martin-Flores, J., Petit-Romec, A., & Scott, A. (2018). Distracted investors and earnings management. *SSRN eLibrary*. Working paper; Available at SSRN: https://dx.doi.org/10.2139/ssrn.3023754.

Gilje, E., Gormley, T. A., & Levit, D. (2018). Who's paying attention? measuring common ownership and its impact on managerial incentives. *SSRN eLibrary*. Available at SSRN: https://ssrn.com/abstract=3165574 or http://dx.doi.org/10.2139/ssrn.3165574.

Gneezy, U., Leonard, K. L., & List, J. A. (2009). Gender differences in competition: Evidence from a matrilineal and a patriarchal society. *Econometrica*, 77(5), 1637–1664.

Goldin, C. (2014). A grand gender convergence: Its last chapter. *American Economic Review*, 104(4), 1091–1119.

Goldin, C. & Rouse, C. (2000). Orchestrating impartiality: The impact of" blind" auditions on female musicians. *American economic review*, 90(4), 715–741.

Graham, S. J., Marco, A. C., & Miller, R. (2018a). The USPTO patent examination research dataset: A window on patent processing. *Journal of Economics & Management Strategy*, 27(3), 554–578.

Graham, S. J., Marco, A. C., & Miller, R. (2018b). The uspto patent examination research dataset: A window on patent processing. *Journal of Economics & Management Strategy*, 27(3), 554–578.

Groysberg, B., Healy, P. M., & Maber, D. A. (2011). What drives sell-side analyst compensation at high-status investment banks? *Journal of Accounting Research*, 49(4), 969–1000.

Hall, B. H., Jaffe, A., & Trajtenberg, M. (2005). Market value and patent citations. *RAND Journal of economics*, (pp. 16–38).

Harford, J., Jiang, F., Wang, R., & Xie, F. (2018). Analyst career concerns, effort allocation, and firms' information environment. *Review of Financial Studies,*. doi:10.1093/rfs/hhy101.

Hartzell, J. C. & Starks, L. T. (2003). Institutional investors and executive compensation. *Journal of Finance*, 58(6), 2351–2374.

Hedges, L. V. & Nowell, A. (1995). Sex differences in mental test scores, variability, and numbers of high-scoring individuals. *Science*, 269(5220), 41–45.

Hellerstein, J. K., Neumark, D., & Troske, K. R. (2002). Market forces and sex discrimination. *The Journal of Human Resources*, 37(2), 353–380.

Hirshleifer, D. (2008). Psychological bias as a driver of financial regulation. *European Financial Management*, 14(5), 856–874.

Hirshleifer, D. & Teoh, S. H. (2010). Psychological influences on financial regulation and policy. *Behavioral Finance: Investors, Corporations, and Markets. Hoboken: Wiley*, (pp. 151–167).

Hoberg, G. & Phillips, G. (2010). Product market synergies and competition in mergers and acquisitions: A text-based analysis. *Review of Financial Studies*, 23(10), 3773–3811.

Holström, B. R. (1989). Agency costs and innovation. *Journal of Economic Behavior & Organization*, 12(3), 305–327.

Hong, H. & Kacperczyk, M. (2010). Competition and bias. *Quarterly Journal of Economics*, 125(4), 1683–1725.

Hong, H., Lim, T., & Stein, J. C. (2000). Bad news travels slowly: Size, analyst coverage, and the profitability of momentum strategies. *Journal of Finance*, 55(1), 265–295.

Huber, K., Lindenthal, V., & Waldinger, F. (2019). *Discrimination, managers, and firm performance: Evidence from "Aryanizations" in Nazi Germany.* Working Paper No. DP13089, CEPR.

Hunt, J. (2016). Why do women leave science and engineering? *ILR Review*, 69(1), 199–226.

Hyde, J. S. & Mertz, J. E. (2009). Gender, culture, and mathematics performance. *Proceedings of the National Academy of Sciences*, 106(22), 8801–8807.

Jaffe, A. B. & Lerner, J. (2011). *Innovation and its discontents: How our broken patent system is endangering innovation and progress, and what to do about it.* Princeton University Press.

Jannati, S., Kumar, A., Niessen-Ruenzi, A., & Wolfers, J. (2018). In-group bias in financial markets. *Available at SSRN 2884218.*

Jensen, K., Kovács, B., & Sorenson, O. (2018). Gender differences in obtaining and maintaining patent rights. *Nature Biotechnology*, 36(4), 307–309.

Jensen, M. C. & Meckling, W. H. (1976). Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of Financial Economics*, 3(4), 305–360.

Jobs, S. P., Forstall, S., Christie, G., Lemay, S. O., Herz, S., Van Os, M., Ording, B., Novick, G., Westerman, W. C., Chaudhri, I., Coffman, P. L., Kocienda, K., Ganatra, N. K., Anzures, F. A., Wyld, J. A., Bush, J., Matas, M., Marcos, P. D., Pisula, C. J., King, V. S., Blumenberg, C., Tolmasky, F. R., Williamson, R., Boule, A. M. J., & Lamiraux, H. C. (2009). Touch screen device, method, and graphical user interface for determining commands by applying heuristics. US Patent 7,479,949.

Kacperczyk, M., Van Nieuwerburgh, S., & Veldkamp, L. (2016). A rational theory of mutual funds' attention allocation. *Econometrica*, 84(2), 571–626.

Kawaguchi, D. (2007). A market test for sex discrimination: Evidence from Japanese firm-level panel data. *International Journal of Industrial Organization*, 25(3), 441–460.

Kempf, E., Manconi, A., & Spalt, O. (2016). Distracted shareholders and corporate actions. *Review of Financial Studies*, 30(5), 1660–1695.

Kogan, L., Papanikolaou, D., Seru, A., & Stoffman, N. (2017). Technological innovation, resource allocation, and growth. *The Quarterly Journal of Economics*, 132(2), 665–712.

Kuhn, J. M. (2016). Property rights and frictions in the sale of patents. *Available at SSRN 2865293.*

Kuhn, J. M. & Thompson, N. C. (2019). How to measure and draw causal inferences with patent scope. *International Journal of the Economics of Business*, 26(1), 5–38.

Kumar, A. (2010). Self-selection and the forecasting abilities of female equity analysts. *Journal of Accounting Research*, 48(2), 393–435.

Kumar, A., Niessen-Ruenzi, A., & Spalt, O. G. (2015). What's in a name? Mutual fund flows when managers have foreign-sounding names. *The Review of Financial Studies*, 28(8), 2281–2321.

Lax Martínez, G., Raffo, J., & Saito, K. (2016). *Identifying the Gender of PCT inventors.* WIPO Economic Research Working Papers 33, World Intellectual Property Organization - Economics and Statistics Division.

Lemley, M. A. & Sampat, B. (2012). Examiner characteristics and patent office outcomes. *Review of Economics and Statistics*, 94(3), 817–827.

Li, G.-C., Lai, R., D' Amour, A., Doolin, D. M., Sun, Y., Torvik, V. I., Amy, Z. Y., & Fleming, L. (2014). Disambiguation and co-authorship networks of the US patent inventor database (1975–2010). *Research Policy*, 43(6), 941–955.

Lichtman, D. (2004). Rethinking prosecution history estoppel. *The University of Chicago Law Review*, (pp. 151–182).

Lins, K. V., Roth, L., Servaes, H., & Tamayo, A. (2020). Gender, culture, and firm value: Evidence from the harvey weinstein scandal and the# metoo movement. *University of Alberta School of Business Research Paper*, (2019-509).

Liu, C., Low, A., Masulis, R. W., & Zhang, L. (2017). Monitoring the monitor: Distracted institutional investors and board governance. *SSRN eLibrary.*

Ljungqvist, A., Marston, F., Starks, L. T., Wei, K. D., & Yan, H. (2007). Conflicts of interest in sell-side research and the moderating role of institutional investors. *Journal of Financial Economics*, 85(2), 420–456.

Malmendier, U. (2018). *Behavioral Corporate Finance.* Working Paper 25162, National Bureau of Economic Research.

Manso, G. (2011). Motivating innovation. *Journal of Finance*, 66(5), 1823–1860.

Marianne, B. (2011). New perspectives on gender. In *Handbook of labor economics*, volume 4 (pp. 1543–1590). Elsevier.

Merges, R. P. (2001). As many as six impossible patent before breakfast: Property rights for business concepts and patent system reform. *Berkeley Technology Law Journal*, 14, 577–615.

Merges, R. P. & Nelson, R. R. (1990). On the complex economics of patent scope. *Columbia Law Review*, 90(4), 839–916.

Merges, R. P. & Nelson, R. R. (1994). On limiting or encouraging rivalry in technical progress: The effect of patent scope decisions. *Journal of Economic Behavior & Organization*, 25(1), 1–24.

Merkley, K., Michaely, R., & Pacelli, J. (2017). Does the scope of the sell-side analyst industry matter? An examination of bias, accuracy, and information content of analyst reports. *Journal of Finance*, 72(3), 1285–1334.

Miyake, A., Kost-Smith, L. E., Finkelstein, N. D., Pollock, S. J., Cohen, G. L., & Ito, T. A. (2010). Reducing the gender achievement gap in college science: A classroom study of values affirmation. *Science*, 330(6008), 1234–1237.

Moser, P. (2005). How do patent laws influence innovation? Evidence from Nineteenth-century world's fairs. *American Economic Review*, 95(4), 1214–1236.

Moser, P. (2013). Patents and innovation: Evidence from economic history. *Journal of Economic Perspectives*, 27(1), 23–44.

Mullen, B., Brown, R., & Smith, C. (1992). Ingroup bias as a function of salience, relevance, and status: An integration. *European Journal of Social Psychology*, 22(2), 103–122.

Murray, F. & Graham, L. (2007). Buying science and selling science: Gender differences in the market for commercial science. *Industrial and Corporate Change*, 16(4), 657–689.

Neumark, D., Bank, R. J., & Van Nort, K. D. (1996). Sex discrimination in restaurant hiring: An audit study. *The Quarterly journal of economics*, 111(3), 915–941.

Niessen, A. & Ruenzi, S. (2007). *Sex matters: Gender differences in a professional setting.* CFR Working Paper 06-01, Cologne.

Nordhaus, W. D. (1969). *Invention, Growth, and Welfare: A Theoretical Treatment of Technological Change and Economic Welfare.* Cambridge, MA: MIT Press.

O'Brien, P. C. & Bhushan, R. (1990). Analyst following and institutional ownership. *Journal of Accounting Research*, 28, 55–76.

Okonofua, J. A. & Eberhardt, J. L. (2015). Two strikes: Race and the disciplining of young students. *Psychological Science*, 26(5), 617–624.

Payne, B. K., Vuletich, H. A., & Brown-Iannuzzi, J. L. (2019). Historical roots of implicit bias in slavery. *Proceedings of the National Academy of Sciences*, 116(24), 11693–11698.

Quinn, G. (2015). For patent owners patent quality is all about value.

Reuben, E., Sapienza, P., & Zingales, L. (2014). How stereotypes impair women's careers in science. *Proceedings of the National Academy of Sciences*, 111(12), 4403–4408.

Sampat, B. & Williams, H. L. (2019). How do patents affect follow-on innovation? Evidence from the human genome. *American Economic Review*, 109(1), 203–36.

Sarsons, H. (2017). Interpreting signals in the labor market: Evidence from medical referrals.

Seymore, S. B. (2006). My patent, your patent, or our patent-inventorship disputes within academic research groups. *Alb. LJ Sci. & Tech.*, 16, 125.

Shu, T., Tian, X., & Zhan, X. (2019). Patent quality and firm value: Evidence from patent examiners busyness. *Available at SSRN 3137894*.

Sims, C. A. (2003). Implications of rational inattention. *Journal of Monetary Economics*, 50(3), 665–690.

Szymanski, S. (2000). A market test for discrimination in the English professional soccer leagues. *Journal of Political Economy*, 108(3), 590–603.

Tabakovic, H. & Wollmann, T. G. (2018). *From revolving doors to regulatory capture? Evidence from patent examiners*. Working paper, National Bureau of Economic Research.

Tian, X. & Wang, T. Y. (2014). Tolerance for failure and corporate innovation. *Review of Financial Studies*, 27(1), 211–255.

Trajtenberg, M., Henderson, R., & Jaffe, A. (1997). University versus corporate patents: A window on the basicness of invention. *Economics of Innovation and new technology*, 5(1), 19–50.

Tzioumis, K. (2018). Demographic aspects of first names. *Scientific data*, 5, 180025.

United States Government (2013). United States Periodic Report to the Committee on Elimination of Racial Discrimination.

USPTO (1983). *Manual of Patent Examining Procedure*. US Department of Commerce, Patent and Trademark Office.

Weber, A. & Zulehner, C. (2014). Competition and gender prejudice: Are discriminatory employers doomed to fail? *Journal of the European Economic Association*, 12(2), 492–521.

Williams, H. L. (2013). Intellectual property rights and innovation: Evidence from the human genome. *Journal of Political Economy*, 121(1), 1–27.

Wolfers, J. (2006). Diagnosing discrimination: Stock returns and CEO gender. *Journal of the European Economic Association*, 4(2-3), 531–541.

Word, D. L., Coleman, C. D., Nunziata, R., & Kominski, R. (2008). Demographic aspects of surnames from Census 2000. *Unpublished manuscript, Retrieved from http://citeseerx.ist.psu.edu/viewdoc/download.*

CENTER DISSERTATION SERIES

CentER for Economic Research, Tilburg University, the Netherlands

No.	Author	Title	ISBN	Published
579	Julius Rüschenpöhler	Behavioural Perspectives on Subsistence Entrepreneurship in Emerging Markets	978 90 5668 580 5	January 2019
580	Khulan Altangerel	Essays on Immigration Policy	978 90 5668 581 2	January 2019
581	Kun Zheng	Essays on Duration Analysis and Labour Economics	978 90 5668 582 9	January 2019
582	Tatiana Zabara	Evolution of Entrepreneurial Teams in Technology-Based New Ventures	978 90 5668 583 6	February 2019
583	Yifan Yu	Essays on Mixed Hitting-Time Models	978 90 5668 584 3	April 2019
584	Daniel Martinez Martin	Unpacking Product Modularity, Innovation in Distributed Innovation Teams	978 90 5668 585 0	April 2019
585	Katalin Katona	Managed Competition in Practice Lessons for Healthcare Policy	978 90 5668 586 7	April 2019
586	Serhan Sadikoglu	Essays in Econometric Theory	978 90 5668 587 4	May 2019
587	Hoang Yen Nguyen	Emotions and Strategic Interactions	978 90 5668 588 1	May 2019
588	Ties de Kok	Essays on reporting and information processing	978 90 5668 589 8	May 2019
589	Yusiyu Wang	Regulation, Protest, and Spatial Economics	978 90 5668 590 4	June 2019
590	Ekaterina Neretina	Essays in Corporate Finance, Political Economy, and Competition	978 90 5668 591 1	June 2019
591	Ruth Wandhöfer	Technology innovation in Financial Markets: Implications for Money, Payments and Settlement Finality	978 90 5668 592 8	June 2019

No.	Author	Title	ISBN	Published
592	Andinet Worku Gebreselassie	On communicating about taboo social issues in least developed countries: The case of Ethiopia	978 90 5668 593 5	June 2019
593	Filip Bekjarovski	Active Investing	978 90 5668 594 2	June 2019
594	Miguel Sarmiento	Essays on Banking, Financial Intermediation and Financial Markets	978 90 5668 595 9	June 2019
595	Xiaoyin Ma	Essays on Alternative Investements	978 90 5668 596 6	June 2019
596	Victor van Pelt	A Dynamic View of Management Accounting Systems	978 90 5668 597 3	June 2019
597	Shuai Chen	Marriage, Minorities, and Mass Movements	978 90 5668 598 0	July 2019
598	Ben Gans	Stabilisation operations as complex systems: order and chaos in the interoperability continuum	978 90 5668 599 7	July 2019
599	Mulu Hundera	Role Conflict, Coping Strategies and Female Entrepreneurial Success in Sub-Saharan Africa	978 90 5668 600 0	August 2019
600	Hao Hu	The Quadratic Shortest Path Problem – Theory and Computations	978 90 5668 601 7	September 2019
601	Emerson Erik Schmitz	Essays on Banking and International Trade	978 90 5668 602 4	September 2019
602	Olga Kuryatnikova	The many faces of positivity to approximate structured optimization problems	978 90 5668 603 1	September 2019
603	Sander Gribling	Applications of optimization to factorization ranks and quantum information theory	978 90 5668 604 8	September 2019
604	Camille Hebert	Essays on Corporate Ownership and Human Capital	978 90 5668 605 5	October 2019
605	Gabor Neszveda	Essays on Behavioral Finance	978 90 5668 606 2	October 2019

No.	Author	Title	ISBN	Published
606	Ad van Geesbergen	Duurzame schaarste - Een kritische analyse van twee economische duurzaamheids-paradigma's geïnspireerd door de filosofie van Dooyeweerd	978 90 5668 607 9	October 2019
607	Richard T. Mason	Digital Enrollment Architecture and Retirement Savings Decisions: Evidence from the Field	978 90 5668 608 6	November 2019
608	Ron Triepels	Anomaly Detection in the Shipping and Banking Industry	978 90 5668 609 3	November 2019
609	Feng Fang	When performance shortfall arises, contract or trust? A multi-method study of the impact of contractual and relation governances on performance in Public-Private Partnerships	978 90 5668 610 9	November 2019
610	Yasir Dewan	Corporate Crime and Punishment: The Role of Status and Ideology	978 90 5668 611 6	November 2019
611	Mart van Hulten	Aiming for Well-Being through Taxation: A Framework of Caution and Restraint for States	978 90 5668 612 3	December 2019
612	Carlos Sandoval Moreno	Three essays on poverty measurement and risk protection	978 90 5668 613 0	December 2019
613	Harmke de Groot	Core strength or Achilles' heel: Organizational competencies and the performance of R&D collaborations	978 90 5668 614 7	December 2019
614	Peter Brok	Essays in Corporate Finance and Corporate Taxation	978 90 5668 615 4	December 2019
615	Pascal Böni	On the Pricing, Wealth Effects and Return of Private Market Debt	978 90 5668 616 1	December 2019
616	Ana Martinovici	Revealing Attention: How Eye Movements Predict Brand Choice and Moment of Choice	978 90 5668 617 8	December 2019
617	Matjaz Maletic	Essays on international finance and empirical asset pricing	978 90 5668 618 5	January 2020
618	Zilong Niu	Essays on Asset Pricing and International Finance	978 90 5668 619 2	January 2020

No.	Author	Title	ISBN	Published
619	Bjorn Lous	On free markets, income inequality, happiness and trust	978 90 5668 620 8	January 2020
620	Clemens Fiedler	Innovation in the Digital Age: Competition, Cooperation, and Standardization	978 90 5668 621 5	October 2020
621	Andreea Popescu	Essays in Asset Pricing and Auctions	978 90 5668 622 2	June 2020
622	Miranda Stienstra	The Determinants and Performance Implications of Alliance Partner Acquisition	978 90 5668 623 9	June 2020
623	Lei Lei	Essays on Labor and Family Economics in China	978 90 5668 624 6	May 2020
624	Farah Arshad	Performance Management Systems in Modern Organizations	978 90 5668 625 3	June 2020
625	Yi Zhang	Topics in Economics of Labor, Health, and Education	978 90 5668 626 0	June 2020
626	Emiel Jerphanion	Essays in Economic and Financial decisions of Households	978 90 5668 627 7	July 2020
627	Richard Heuver	Applications of liquidity risk discovery using financial market infrastructures transaction archives	978 90 5668 628 4	September 2020
628	Mohammad Nasir Nasiri	Essays on the Impact of Different Forms of Collaborative R&D on Innovation and Technological Change	978 90 5668 629 1	August 2020
629	Dorothee Hillrichs	On inequality and international trade	978 90 5668 630 7	September 2020
630	Roland van de Kerkhof	It's about time: Managing implementation dynamics of condition-based maintenance	978 90 5668 631 4	October 2020
631	Constant Pieters	Process Analysis for Marketing Research	978 90 5668 632 1	December 2020
632	Richard Jaimes	Essays in Macroeconomic Theory and Natural Resources	978 90 5668 633 8	November 2020

No.	Author	Title	ISBN	Published
633	Olivier David Armand Zerbib	Asset pricing and impact investing with pro-environmental preferences	978 90 5668 634 5	November 2020
634	Laura Capera Romero	Essays on Competition, Regulation and Innovation in the Banking Industry	978 90 5668 635 2	December 2020
635	Elisabeth Beusch	Essays on the Self-Employed in the Netherlands and Europe	978 90 5668 636 9	December 2020
636	Sophie Zhou	Essays on the Self-Employed in the Netherlands and Europe	978 90 5668 637 6	November 2020
637	Vincent Peters	Turning modularity upside down: Patient-centered Down syndrome care from a service modularity perspective	978 90 5668 638 3	December 2020
638	Pranav Desai	Essays in Corporate Finance and Innovation	978 90 5668 639 0	January 2021

This Ph.D. dissertation consists of three independent chapters in corporate finance and innovation. The first chapter studies how in-group biases of patent examiners - an important set of regulators - affect their decisions to grant patents. Additionally, it highlights the costs of these biased decisions for inventors, startups, and the economy. The second chapter studies how limited investor attention affects analyst coverage of firms and thereby, information provision to the financial markets. The last chapter documents that firms punish female inventors for their "as-good-as-random" creative failures more harshly than they punish men. These differences in punishment cause early-stage female inventors to exit innovation at higher rates than their male colleagues.

Pranav Pradeep Desai (Mumbai, India, 1990), received his Bachelor's Degree in Commerce from University of Mumbai, and then a Master's Degree in Finance and a Master's Degree in Economics from Lancaster University and University College London respectively. He began his Research Master in Finance at Tilburg University in 2015, supported by the CentER scholarship, before starting his PhD in 2016. In fall 2020, he joined the Department of Finance at Nova School of Business and Economics as an Assistant Professor.

ISBN: 978 90 5668 639 0
DOI: 10.26116/center-lis-2017

TILBURG SCHOOL OF ECONOMICS
AND MANAGEMENT

CentER Dissertation Series